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THE ANNEALING AND RE-ANNEALING OF GLASS

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ABSTRACT. In Part III of this paper the formulae deduced in the previous parts are applied to the case of heating glass under constant gradient and the temperature at which strain disappears is obtained, the effect of the known change in the coefficient of expansion on this temperature being discussed. The calculated and experimental curves are compared and the effect of changes in the rate of heating and in the size of the specimens is considered.

In Part IV the annealing equation is considered from the point of view of dimensions. The agreement of theory and experiment is discussed and an explanation of the discrepancies at low temperatures is advanced. A general expression to cover all cases is deduced and the results of theory and experiment are compared.

PART III. THE DETERMINATION OF ANNEALING TEMPERATURES

Introduction

IT has been suggested and very generally adopted that the annealing temperature of a glass may be determined by heating a small specimen under conditions where the double refraction may be observed and recording the temperature at which the strain disappears. Reference may be made to papers by English and Turner* and Tool and Valasek† for details of the procedure. In the following paper this temperature is calculated in terms of the known physical constants of the glass by means of formulae similar to those derived in the previous paper‡ for the case of cooling.

* English and Turner, *Journ. Soc. Glass Tech.* 2 (1918) 90; 3 (1919) 125.

† Tool and Valasek, *Bur. Stand. Scientific Paper*, No. 358 (1920).

‡ Hampton, "The Annealing and Re-annealing of Glass," Parts I and II, *Trans. Opt. Soc.* 26 (1924-25) 14.

Previously it was shown that the strain (measured in terms of double refraction) present in the glass at any time could be deduced from the relations connecting the disappearance of strain at constant temperature with time and the variation of the velocity constant λ with temperature. The conditions necessary to ensure the final strain being a minimum were considered and numerical data for annealing glass calculated from the corresponding equations.

The present problem is to find the double refraction s_t which remains at any temperature during heating at a constant rate of $+p°$ C. per min. It is assumed that the specimen is originally strain-free and therefore the only strain present is that due to the temperature gradient, i.e. s_0 which equals Dpb^2 (see equation (15)) and the strain at any time $s_t = s_0 - s$, where s is the strain relieved by plastic flow.

It may be assumed for the purposes of calculation that heating commences at $\theta_0 - 100$ as no strain is relieved by flow below this temperature. Thus the temperature at any time

$$= \theta_0 - 100 + pt$$

and $$\lambda = \lambda_0 2^{\frac{pt-100}{d}} \text{ from (1).}$$

Then from (4)

$$\frac{ds_t}{dt} = -\lambda_0 2^{\frac{pt-100}{d}} s_t^2$$

and $s_t = s_0$ when $t = 0$ or

$$\frac{1}{s_0 - s} - \frac{1}{s_0} = \frac{d\lambda_0}{p \log_e 2} 2^{-\frac{100}{d}} \left(2^{\frac{pt}{d}} - 1\right) \qquad \text{.....(21).}$$

But $s_0 = Dpb^2$, whence

$$\frac{s_0 - s}{s_0} = \frac{1}{1 + \dfrac{\lambda_0 dDb^2}{\log_e 2 \,.\, 2^{\frac{100}{d}}} \left(2^{\frac{pt}{d}} - 1\right)} \qquad \text{.....(22),}$$

where $\lambda_0 = 1$.

In (22), since pt represents a temperature, it appears that alterations in the rate of heating will not affect the ratio

$$\frac{s_0 - s}{s_0},$$

but will alter the absolute value of $s_0 - s$ at a given temperature. Changes in the size of specimen will affect both their ratio and the value $s_0 - s$.

Application to a Particular Case

Consider a slab of flint glass of thickness $2b$. Then from Tables I and II

$$d = 8,\ D = \cdot 025, \quad \text{and} \quad \frac{Dd}{\log_e 2} = \cdot 40,$$

whence

$$\frac{s_0 - s}{s_0} = \frac{1}{1 + \cdot 000050 b^2 2^{\frac{pt}{8}} - 1} \qquad \text{.....(23).}$$

Values calculated from this equation are given in Table VIII.

Table VIII. *Values, calculated from* (23), *giving the strain remaining at any temperature relative to the strain due to the imposed gradient* ($b = 1$)

Temperature relative to θ_0	$\frac{\epsilon_0 - \epsilon}{\epsilon_0}$
$\theta_0 - 100$	1·00
$\theta_0 - 80$	·999
$\theta_0 - 60$	·993
$\theta_0 - 40$	·970
$\theta_0 - 20$	·893
θ_0	·714
$\theta_0 + 20$	·414
$\theta_0 + 40$	·165
$\theta_0 + 60$	·054
$\theta_0 + 80$	·016
$\theta_0 + 100$	·004
$\theta_0 + 120$	·0013

When these values are compared with experimental values it is found that the calculated values do not decrease with sufficient rapidity, indicating that some important factor has been ignored.

Turning to the derivation of D in (14) it is seen that D includes the coefficient of expansion α considered as a constant independent of temperature. It has been shown, however, that there is a sudden increase in α at or above the annealing range*. Peters and Cragoe give curves from which the value of α at any temperature may be deduced. Fig. 2 is taken from Fig. 9 (p. 128) of their paper.

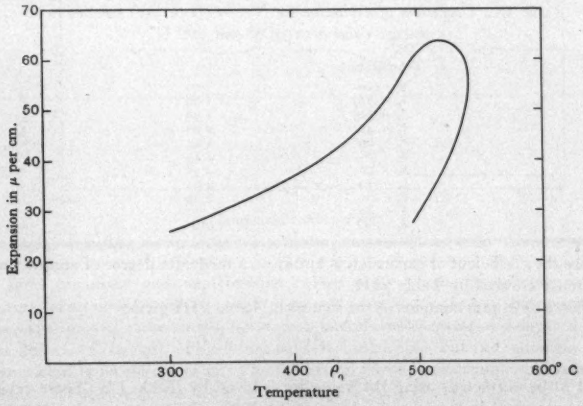

Fig. 2. Expansion Curve for M.F. (No. 12) from Peters and Cragoe's paper

* Lebedeff, *Trans. Opt. Inst. Petrograd*, 2 (1921) 10; Abstr. A. 114, *Journ. Soc. Glass Tech.* (1922); Pietenpol, *Chem. and Met. Eng.* 23 (1920) 876; Lafon, *Compt. Rend.* 175 (1922) 955; Peters and Cragoe, *Journ. Opt. Soc. Amer.* 4 (1920) 105.

Their glass, No. 12 (B.S. 110), is similar to Adams and Williamson's M.F. Table IX gives values of α deduced from Fig. 2. These values are plotted in Fig. 3 against temperatures relative to θ_0, which may be deduced from other data in a paper published by Tool and Valasek* to be about 430° C.

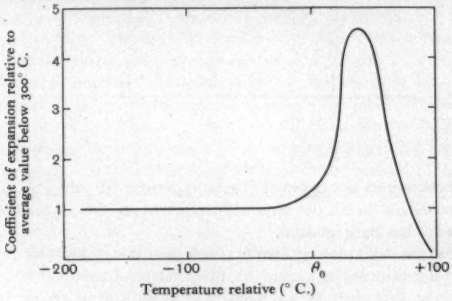

Fig. 3. Values of Coefficient of Expansion deduced from Fig. 2

Table IX. *Coefficient of expansion for No.* 12 (*B.S.* 110) *relative to the average value between* 0° *and* 300° *C.*

Temperature	α
$\theta_0 - 100$	1·00
$\theta_0 - 80$	1·00
$\theta_0 - 60$	1·00
$\theta_0 - 40$	1·06
$\theta_0 - 20$	1·22
θ_0	1·43
$\theta_0 + 20$	2·55
$\theta_0 + 40$	4·60
$\theta_0 + 60$	2·65
$\theta_0 + 80$	·87
$\theta_0 + 100$	nil

Hence the coefficient of expansion is known to a moderate degree of accuracy over the range covered by Table VIII.

Since D in (22) contains α, the figures in Table VIII giving

$$\frac{s_0 - s}{s_0}$$

need to be corrected, using the value for α given by Table IX. These revised values are given in the second column of Table X. The figures need to be multiplied again by the revised value of α as s_0 is itself proportional to α and the third column of Table X gives the final values for the disappearance of strain.

* Tool and Valasek, *loc. cit.*

Table X. *Rate of disappearance of strain using corrected values for α together with equation* (22)

Temperature	Revised value of $\frac{\varepsilon_0 - \varepsilon}{\varepsilon_0}$	Rate of disappearance of strain
$\theta_0 - 100$	1·00	1·00
$\theta_0 - 80$	·999	·999
$\theta_0 - 60$	·993	·993
$\theta_0 - 40$	·970	1·028
$\theta_0 - 20$	·880	1·073
θ_0	·638	·913
$\theta_0 + 20$	·217	·554
$\theta_0 + 40$	·042	·193
$\theta_0 + 60$	·021	·055
$\theta_0 + 80$	·018	·015
$\theta_0 + 100$	—	—

These final calculated figures are plotted in Fig. 4 and in the same figure is a curve copied from Tool and Valasek's paper for the disappearance of strain in a cylinder of B.S. 110 during heating at constant rate.

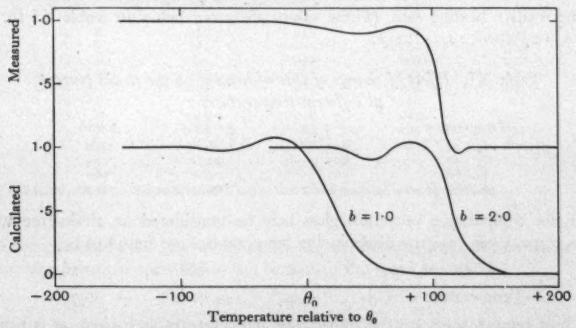

Fig. 4. Calculated and Measured Curves showing Strain remaining at any Temperature

Both calculated and experimental curves show similar phenomena in the decrease of strain followed by a slight increase before the final rapid disappearance. The experimental curve shows, however, a lateral displacement as compared with that deduced from (22). This is not entirely explainable, but the following facts have a bearing on the question. The theoretical curve shows complete absence of strain at about $\theta_0 + 80$, i.e. for B.S. 110 at $430 + 80 = 510°$ C.

Tool and Valasek in their summary on p. 540 of their paper give $510 \pm 15°$C. as the upper annealing temperature determined by an optical method. This value is in excellent agreement with the one deduced above but does not agree with that given in Tool and Valasek's detailed curve.

It should be noticed that the temperatures in the summary were determined by a thermocouple inserted in a small hole along the axis of the cylinder (p. 541), thus giving the true temperature.

In a letter to the writer from the Director of the Bureau of Standards it was stated that the temperatures given in the curve quoted were measured at the *surface* of the glass. He also said that of necessity a relatively large specimen and high rate of heating were employed. This implies that a large temperature gradient existed across the specimen dealt with in their curve and the point of disappearance was thereby apparently shifted along the temperature axis. No allowance for this is made in the above calculation, hence quantitative temperature agreement cannot be expected between the results of calculation and experiment (see, however, later on the effect of size of specimen).

The Effect of Changes in the Rate of Heating on the Disappearance Temperature

From (22) it is seen that $s_0 - s$ is proportional to s_0 for a specimen of any size, and therefore $s_0 - s$ is proportional to p from (15). The values of $s_0 - s$ may therefore be deduced from Table X by multiplying the figures in the third column by the relative heating rate. These values are given below in Table XI for the case $b = 1$, whence $s_0 = \cdot 025p$.

Table XI. *Effect of change of rate of heating on the strain present at different temperatures*

Temperature	$p=4$	$p=8$	$p=16$
θ_0+20	·025	·100	·400
θ_0+40	·005	·021	·084
θ_0+60	·001	·005	·020
θ_0+80	—	·0007	·0028

If, for comparative purposes, glass may be considered as strain-free when $s_0 - s = \cdot 005\lambda$ per cm., the disappearing temperatures are found to be

p	4	8	16
Temperature $= \theta_0$	$+40°$	$+60°$	$+76°$

These temperatures are the actual glass temperatures and when, as is usually the case in practice, the *surface* temperature is measured, a correction for the temperature gradient in the glass is necessary. This gradient may be shown to be*

$$\theta' = -\frac{p}{2K}(b^2 - x^2),$$

where θ' = temperature difference between centre and point x from the centre,

K = thermal diffusivity.

For the case $x = 0$ in a slab of flint glass this becomes

$$\theta' = -2{\cdot}2pb^2,$$

which gives for the above cases 8°, 16°, and 32° as corrections to be added to the

* See Adams and Williamson, *Journ. Franklin Inst.* 190 (1920) 858, equation (39).

true temperatures to give the apparent disappearing temperatures. The final figures are then

$+56°$ $+76°$ $+108°$

Experimental values determined on slabs 2 cm. thick were

$+55°$ $+70°$ $+86°$

The Effect of the Size of the Specimen on the Disappearance Temperature

Table XII gives the results of numerical computation from (22) for various values of b assuming a value of 8° per min. for p. s_0 is again taken as $\cdot 20b^2$ (see Table XI).

Table XII. *Effect of altering the size of the specimen on the strain present at various temperatures*

	b			
Temperature	·25	·50	1·0	2·0
$\theta_0 - 100$	·012	·050	·200	·400
$\theta_0 - 80$	·012	·050	·200	·400
$\theta_0 - 60$	·012	·050	·200	·360
$\theta_0 - 40$	·014	·056	·205	·436
$\theta_0 - 20$	·017	·068	·229	·440
θ_0	·017	·065	·200	·220
$\theta_0 + 20$	·026	·062	·100	·060
$\theta_0 + 40$	·016	·020	·021	·012
$\theta_0 + 60$	·004	·009	·005	·002
$\theta_0 + 80$	·0006	·0007	·0007	—

The disappearance temperatures from the above table are therefore

$+58$ $+68$ $+60$ $+52$

After correction for the temperature lag in the specimen the apparent disappearance points as recorded at the surface of the specimen will be

$+59$ $+72$ $+76$ $+112$

An experimental determination on a sample of flint (D.F. 4273) similar in composition to the M.F. of Adams and Williamson gave the following values:

b	·50	1·00	2·00
θ_0	$+70$	$+79$	$+140$

The determination for the largest specimen was probably vitiated by the fact that it nearly filled the furnace.

The relative values for $b = 2{\cdot}0$ after correction for temperature-lag are plotted in Fig. 4. The agreement between Tool and Valasek's curve and the calculated graph is now much better and it seems reasonable to suppose that the discrepancies previously noted are to be ascribed to the uncertainty of the dimensions of their specimen.

Conclusions

The theoretical considerations brought forward point to the rather remarkable conclusion that the so-called annealing temperature can only be measured at all because of the sudden change of the coefficient of expansion in the region where measurable viscous flow is commencing. There would, without this abrupt change in expansion, be only a gradual dying away in strain which would be too slow to enable anything approaching a definite temperature to be measured.

The temperature itself is of use for comparing the relative annealing requirements of similar glasses but would not be suitable as a basis for annealing widely different types.

To obtain repeatable results it is necessary to use specimens of a standard size heated at a standard rate, though the temperature differences introduced by ignoring this are chiefly due to temperature lag and not to an actual variation in the true temperature.

Part IV. The Annealing of Glass at Low Temperatures

Introduction

The general annealing equation (4) as given in Part I is

$$\frac{ds}{dt} = -\lambda s^2,$$

which leads to equation (5)

$$\frac{1}{s} - \frac{1}{s_0} = \lambda t.$$

In the above s is measured in terms of the birefringence produced by the stress f in the glass and the two quantities are connected by the stress-optical coefficient σ so that $s = \sigma f$.

Preston* has shown by means of dimensional analysis that, if the rate of change of stress depends only on the stress present and the viscosity,

$$\frac{df}{dt} = \frac{k}{\eta} f^2 \qquad \text{.....(24),}$$

where

η = viscosity,

f = stress,

k = constant.

This equation is exactly similar to (4) above where λ is proportional to $1/\eta$, i.e. the fluidity of the material. This equation (24) is probably correct for a *perfect* fluid, i.e. a fluid possessing viscosity but not rigidity. At high temperatures glasses probably approximate to this state.

* Preston, "The Fundamental Law of Annealing," *Trans. Opt. Soc.* 26 (1924–25) 270.

The general differential equation relating to the flow of a viscous body entirely free from rigidity may be shown to be

$$\frac{d^2v}{dx^2} = \frac{\rho}{\eta}\frac{dv}{dt},$$

where ρ = density,

v = velocity of flow,

x = distance from a datum plane,

t = time.

On putting in suitable boundary conditions this leads to a solution of the form

$$v = \frac{V}{X}x - \frac{V}{X}\int_0^x e^{\frac{2\eta}{\rho}\cdot\frac{t}{c^2-x^2}}\,dx.$$

This equation is not integrable by any but graphical methods, but the use of such yields a curve, connecting flow with time, which is *convex* towards the time axis. No actual substance has been shown to conform to this condition, all actual materials giving a curve of the opposite type. Thus pitch* shows a flow-time curve concave towards the time axis. This is due to—what is otherwise obvious from considerations of fracture phenomena, for instance—the fact that viscous bodies still have a rigidity or pseudo-rigidity which may or may not be independent of the stress. Such rigidity would decrease with rise of temperature.

Preston also shows that, if the substance possesses rigidity as well as viscosity,

$$\frac{df}{dt} = \frac{k}{\eta}f^2F\left(\frac{f}{G}\right) \qquad \text{.....(25),}$$

where $F\left(\frac{f}{G}\right)$ is an unknown function and G = rigidity.

He suggests that this unknown function may be the explanation of the failure of equation (24) to cover the facts at relatively low temperatures.

It has not been found possible to take into account all the above considerations but an equation is developed later which, while consistent with dimensional requirements and numerical results, gives what appears to be a statistical approximation to the real unknown law.

The Agreement of Equation (24) *with the Results of Experiment*

In order to check the equation it is necessary to assume that Hooke's law holds exactly for glass at fairly high temperatures, since the double refraction measured is taken as proportional to the stress, the ratio being σ, the stress optical coefficient. Savur† has shown that strict proportionality exists between double refraction and stress for large variations of stress and for stresses of opposite sign at room temperatures.

Adams and Williamson‡ give many results showing that at relatively high

* Trouton, *Proc. Roy. Soc.* 77 (1906) 426.

† Savur, *Phil. Mag.* 50 (1925) 453–463.

‡ Adams and Williamson, *Journ. Franklin Inst.* 190 (1920) 597, 835.

temperatures the disappearance of double refraction with time is in agreement with the law expressed in equation (24).

The author has also carried out, during determinations for works purposes, many series of experiments which confirm these results. A few typical results are given in Fig. 5 for B.L.F. 5470, the time and stresses being expressed in C.G.S. units instead of in terms of double refraction for reasons which appear below (σ for B.L.F. 5470 = $\cdot 50 \times 10^{-8}$ wave-length per cm. length per dyne per cm.^{2}).

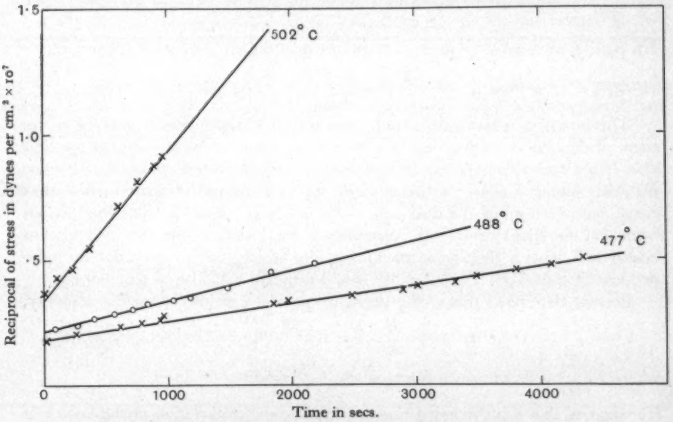

Fig. 5. Curve showing Relation between Stress and Time for B.L.F. 5470

Assuming equation (24), the relation between f_0 (the stress at time $t = 0$) and f is given by

$$\frac{1}{f} - \frac{1}{f_0} = \frac{k}{\eta} t \qquad \text{.....(26).}$$

From the curves in Fig. 1 the values of k/η may be deduced and the results are given in Table XIII for two cases, B.L.F. 5470 and D.F. 5431 ($\sigma = \cdot 50 \times 10^{-8}$).

Table XIII

B.L.F. 5470		D.F. 5431	
Temperature °C.	$\frac{k}{\eta} \times 10^{11}$	Temperature °C.	$\frac{k}{\eta} \times 10^{11}$
476	·65	399	·71
477	·75	417	2·65
485	1·25	427	7·1
488	1·65	438	10·1
502	5·75	483	228·0
507	8·0		
513	9·0		

The Value of k

To decide what value is to be attributed to k it is necessary to measure η by some independent means. A number of methods have been proposed at various times among which may be mentioned the sagging of beams*, the twisting of rods†, and the drawing out of rods*‡.

For the present range it was decided to adopt the method of measuring the rate of sag of beams of various sizes under loads suitable to the temperature. For the lower temperatures (η about 10^{12}) strips about $5 \times 1 \times \cdot 2$ cm. were used as cantilevers with an end-load of about 15 grams. For values of η about 10^{10} to 10^{11} cantilevers of $2 \times 1 \times \cdot 25$ cm. were used with the same load, and for $\eta = 10^{8}$ to 10^{10} cantilevers of $2 \times 1 \times \cdot 25$ cm. were allowed to sag under their own weight.

These strips were clipped in a small stand of nickel (see Fig. 6) and placed in a tubular electric furnace through which a beam of light could be passed. An image of the end of the strip was projected on to a ground glass screen giving a magnification of about 4. The furnace was run up to the pre-determined temperature and then maintained at a constant temperature, a record of this control

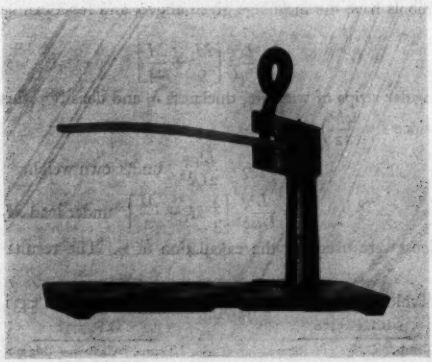

Fig. 6

being taken by a Callendar recorder calibrated in freezing zinc. Control was possible to about $\pm 1 \cdot 5^{\circ}$ C. by means of adjustable resistances. The rates of sag were usually between $\cdot 00005$ and $\cdot 001$ cm. per second, the average rate of sag over several minutes being measured when the beam was approximately horizontal. The actual rates as seen on the screen were thus about $\cdot 1$ to $2 \cdot 0$ mm. per minute.

* Trouton, *Proc. Roy. Soc.* 77 (1906) 426.

† Trouton and Andrews, *Phil. Mag.* (6) 7 (1904) 347; Stott and others, *Proc. Roy. Soc.* A, 108 (1925) 154.

‡ English, *Journ. Soc. Glass Tech.* 7 (1923) 25.

Calculation of η from the Experimental Data

Trouton* deduces the following formula for the rate of sag at the centre of a beam of viscous material under its own weight when supported at both ends:

$$U = \frac{5}{384}\frac{MgL^3}{lI},$$

where U = rate of sag at centre in cm. per second,

M = mass of rod between supports,

g = gravity,

L = length,

I = moment of inertia,

l = coefficient of viscous traction which is shown to be 3η.

Making the necessary changes for the case of a cantilever under its own weight, this reduces to

$$U = \frac{MgL^3}{8lI} = \frac{MgL^3}{24\eta I} \qquad \text{.....(27),}$$

where the symbols have the meanings given above, and for a cantilever under an end-load M_1

$$U = \frac{L^3 g}{\eta I}\left[\frac{M_1}{9} + \frac{M}{24}\right] \qquad \text{.....(28).}$$

For rectangular strips of width w, thickness b, and density ρ these become on transposing $\left(\text{since } I = \frac{wb^3}{12}\right)$,

$$\eta = \frac{L^4 \rho g}{2Ub^2} \quad \text{under own weight} \qquad \text{.....(29),}$$

$$\eta = \frac{L^3 g}{Uwb^3}\left[\frac{4}{3}M_1 + \frac{M}{2}\right] \quad \text{under load } M_1 \qquad \text{.....(30).}$$

These equations were used for the calculation of η. The results are given in Table XIV.

Table XIV. *Values of η for B.L.F. 5470 and D.F. 5431*

B.L.F. 5470		D.F. 5431	
Temp. ° C.	log η	Temp. ° C.	log η
513	13·00	458	12·25
532	12·50	485	11·46
536	12·34	496	11·00
559	11·60	510	10·52
565	11·38	527	9·91
571	11·47	560	9·24
574	11·30	569	8·90
590	10·89	585	8·54
597	10·41	612	8·02
601	10·21		
645	9·59		
665	9·30		
674	8·90		
695	8·76		
714	8·23		

* Trouton, *Proc. Roy. Soc.* 77 (1906) 426.

These values are plotted in Fig. 7.

It is now possible to deduce the value of k to a fair degree of accuracy from the figures given in Tables XIII and XIV. The values of k are found to be 3·20 for the B.L.F. and 3·90 for the D.F. where k and η are given in terms of logarithms. The viscosities as deduced from the birefringence measurements are given in Table XV.

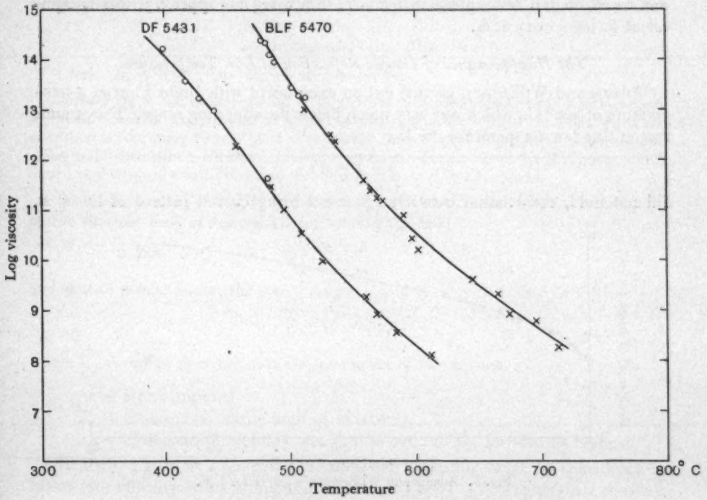

Fig. 7. Curves showing Relation between Viscosity and Temperature

Table XV

B.L.F. 5470		D.F. 5431	
Temp. ° C.	log η	Temp. ° C.	log η
476	14·39	399	14·21
477	14·33	417	13·55
485	14·10	427	13·20
458	13·95	438	12·95
502	13·44	483	11·60
507	13·30		
513	13·25		

These points are plotted on the graphs in Fig. 7 as circles while the points found from bending experiments are given as crosses.

Thus k for B.L.F. = 1600

and k for D.F. = 1000.

The Value of η at θ_0

From the data in Table XIII it may be deduced that

B.L.F. 5470 $\theta_0 = 507°$ C. Log η at this temperature = 13·30.
D.F. 5431 $\theta_0 = 430°$ C. ,, ,, = 13·20.

Preston* deduced a value of $\log \eta = 10$ from the author's previous data†, but this was based on the assumption that $k = 1$. Since $\log k$ = approx. 3, this is equivalent to $\log \eta = 13$ at θ_0.

The Disappearance of Double Refraction at Low Temperatures

Adams and Williamson carried out an experiment with Light Flint at a temperature of 300° C., which was very much below the annealing range. They found that at this low temperature the law

$$\frac{1}{f} - \frac{1}{f_0} = k't,$$

did not hold, the relation between $1/f$ and t being curved instead of linear as

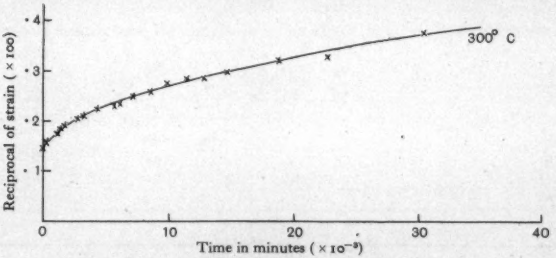

Fig. 8. Adams and Williamson's Curve for L.F.

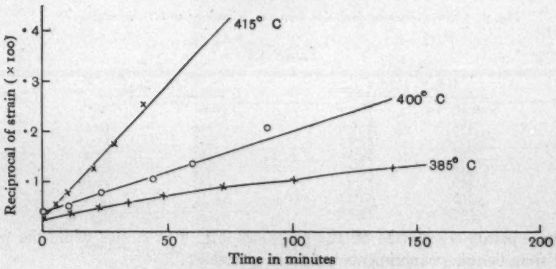

Fig. 9. Adams and Williamson's Curves for L.F.

* Preston, *loc. cit.*

† Hampton, *Trans. Opt. Soc.* 26 (1924–25) 14, Part I.

indicated by the equation. If these results are plotted as in Figs. 8 (which is copied from their paper) and 9, it is apparent that at high temperatures there are indications of this curvature which appears greater than the possible experimental error. Thus at 385° the curve is distinctly concave towards the time axis but at 415° it is sensibly straight. This seems to indicate that at lower temperatures the disappearance of birefringence is not a purely *viscous* phenomenon. An explanation is possible, however, in terms of the known phenomena of plasticity.

Viscous and Plastic Flow

Clerk Maxwell's* views were stated as follows:

If the form of the body is found to be permanently altered when the stress exceeds a certain value, the body is said to be soft or plastic and the state of the body when the alteration is just going to take place is called the limit of perfect elasticity. If the stress, when it is maintained constant, causes a strain or displacement in the body which increases continually with the time, the substance is said to be viscous.

Bingham† makes this idea the basis of his classification of soft bodies and shows that the flow of viscous bodies follows the law

$$\frac{dv}{dr} = \frac{F}{\eta},$$

and that of plastic bodies the law

$$\frac{dv}{dr} = (F - F_L)\mu,$$

where $\frac{dv}{dr}$ = rate of flow per unit distance apart of two planes,

F = stress imposed,

F_L = constant (= elastic limit of Maxwell),

μ = coefficient of mobility and is analogous to $1/\eta$ for viscous bodies.

If, then, a glass at a temperature below the annealing range be considered as plastic, the effective stress acting in it will be not f but $f - f_L$ where f_L is a constant which will probably depend only on the temperature. Thus, when a glass is annealing, the stress will be relieved by flow until it reaches a value f_L which is not necessarily zero, that is

$$\frac{df}{dt} = \frac{k}{\eta}(f - f_L)^2 \qquad \text{.....(31),}$$

or

$$\frac{1}{f - f_L} - \frac{1}{f_0 - f_L} = \frac{kt}{\eta} \qquad \text{.....(32).}$$

The application of this equation to Adams and Williamson's results for L.F. gives a straight line when

$$\begin{aligned} f_L &= 6{\cdot}4 \times 10^6 \text{ at } 300°, \\ &= {\cdot}8 \times 10^6 \text{ at } 385°, \\ &= 0 \quad\quad\quad \text{at } 400°, \\ &= 0 \quad\quad\quad \text{at } 415°, \end{aligned}$$

* Clerk Maxwell, *Theory of Heat* (1877) 295.

† Bingham, *Fluidity and Plasticity* (1922) pp. 215 *et seq.* (McGraw-Hill Book Co.).

the stress being measured in dynes per cm.^{2} and the time in seconds. It must not be assumed from this statement that f_L is really zero at these temperatures but that it simply becomes sufficiently small to be undetectable by the method of experiment used. (In Adams and Williamson's units these values are 200 at 300°, and 25 at 385°.)

Figs. 10 and 11 give the results of plotting $f - f_L$ against time, where f is the experimental figure. The agreement is remarkably good and suggests that glass becomes plastic as it cools. Thus this substance passes through the whole gamut from viscous via plastic to elastic states as it cools from a high temperature.

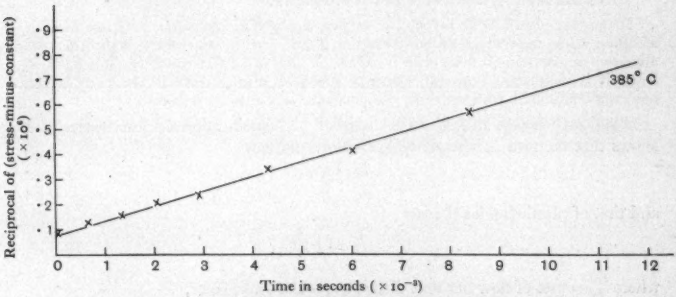

Fig. 10. Curve showing Relation between Stress-minus-Constant and Time (Adams and Williamson's L.F.)

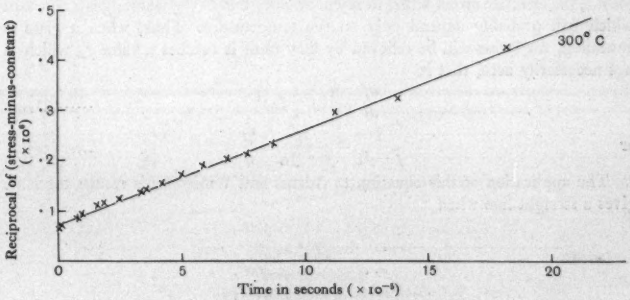

Fig. 11. Curve showing Relation between Stress-minus-Constant and Time (Adams and Williamson's L.F.)

Twyman's Results on Chilling and Tempering

An interesting light is thrown on these conclusions by Twyman's results*. It is known that glass strained by rapid cooling is of lower refractive index than well-annealed glass. It can also be shown that the depression of the index is proportional to the double refraction. Twyman showed that long heating at temperatures below the annealing range of glass so strained caused the index to rise to a definite value which was below that of glass that had been properly annealed at a higher temperature. In other words, since the depression of index is proportional to double refraction and hence to stress, some of the stress could not be removed by maintenance at a temperature which is low relative to the annealing range; this is in exact accordance with the idea in equation (32).

T. Smith† suggested that the data given by Adams and Williamson indicated to him that the real law of annealing involved not an ultimate state in which the strain was absent, but the decay of the strain to a finite limiting value which depended on the temperature. This view is consistent with the facts and also correlates the behaviour of glass with the properties of clays and paints as studied by Bingham and Green‡.

The Viscosity Curve of Lead Glasses

If k in equation (32) be taken as 10^3, which is the average value found for the two glasses studied in detail, then the following values may be deduced from Adams and Williamson's results for D.F. and M.F. on the disappearance of double refraction. This deduction is possible since values are given in their paper for the stress optical coefficient.

Table XVI

L.F.		M.F.	
Temp. ° C.	log η	Temp. ° C.	log η
300	15·72	306	15·76
385	13·23	374	14·20
400	12·97	390	13·56
415	12·04	406	12·92

The values of θ_0 for these glasses§ are: L.F. 400 and M.F. 408.

These values and also the values of η for D.F. 5431, which is a glass of similar type (silica, lead, soda, potash), are plotted in Fig. 12 against the corresponding temperatures measured relatively to θ_0. It is seen that all the points fall sensibly on the same curve and that this curve shows a change of direction as the viscosity increases above 10^{13}. A similar point of inflection is noticed by Stott‖ in his curve for the viscosity of glass St. 59 at approximately the same point.

* Twyman and Simeon, "The Chilling and Tempering of Glass," *Journ. Soc. Glass Tech.* 7 (1923) 199.

† Smith, Discussion on Preston's paper, *Trans. Opt. Soc.* 26 (1924–25) 274.

‡ Bingham, *loc. cit.*; Bingham and Green, *Proc. Amer. Soc. Test. Mat.* 19 (1919) 640.

§ Hampton, *loc. cit.*

‖ Stott, *loc. cit.*

Conclusions

It is now possible to review the assumptions made in the earlier paper in order to simplify the mathematical work and to discuss their effect in view of the later experimental data.

It has been shown that the rate of change of stress is proportional to the square of the stress present. This is true dimensionally and agrees well with experiment as long as the yield point of the material is negligibly small. If this cannot be assumed, the stress present must be considered as the stress above the yield point

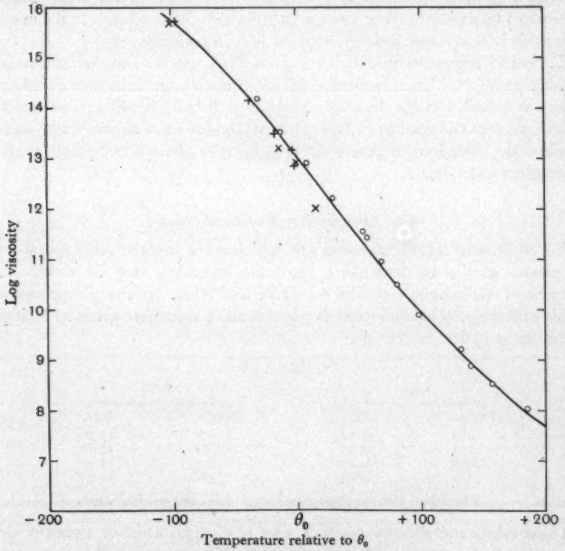

Fig. 12. Viscosity Curve for Flints

when the equation is still true. As the actual yield point is very small at temperatures near the annealing range, this correction will make no practical alteration in the numerical results given in Parts I and II.

The second assumption is that known as Twyman's law, which is equivalent to saying that the logarithm of the viscosity is proportional to temperature. That this is only approximately true is indicated by the viscosity curves given, but over the range of temperatures used in the calculations it is sufficiently close to allow the deductions to stand.

The third assumption relates to the constancy of the coefficient of expansion. The experimental evidence shows that this assumption is true at temperatures below θ_0 so that the numerical results in Parts I and II are not in general affected. The assumption is not made in Part III, as this paper discusses the effect of the variation of α on the disappearance of strain.

There are several other tacit assumptions regarding which there is at present no direct evidence. Thus, it has been of necessity assumed that the stress optical coefficient is a constant independent of temperature; the probability is that this is true, as the agreement between theory and experiments on the disappearance of double refraction during constant heating is good.

It thus seems that the theory outlined in this series of papers is sufficiently closely in agreement with the experimental evidence to give a working basis for the actual annealing of glass. The need now is for more data on such points as the variation of the yield point with temperature, the exact evaluation of k in equation (32), and further determinations of η at various temperatures for different glasses.

The idea of "corresponding states" implied in the plotting of η against temperatures relative to θ_0 for glasses of the same chemical type may be of use in this connexion.

The author's acknowledgements are due to Dr H. Moore for his kindness in reading through this paper and for his helpful criticism.

DISCUSSION

Mr J. Guild: There are two points in connexion with the expansion of optical glass to which I would like to refer. The coefficient is shown on Fig. 3 as having a constant value below the softening temperature, while in the neighbourhood of that temperature it rapidly rises, reaches a maximum, and then diminishes rapidly. In the region below the softening temperature, glass, like most other solids, has a coefficient of expansion which is not constant but is a linear function of temperature, the length of a specimen being given by $l_t = l_0(1 + at + \beta t^2)$, the mean coefficient from 0 to t being $\alpha + \beta t$ while the coefficient at the temperature t is $\alpha + 2\beta t$. Values for certain typical glasses which I had occasion to measure about six years ago, using a method described more recently in the *Journal of Scientific Instruments** were, for a particular hard crown,

$$\alpha = 7{\cdot}8 \times 10^{-6};\ \beta = {\cdot}0034 \times 10^{-6};$$

for a particular flint $\alpha = 8{\cdot}9 \times 10^{-6};\ \beta = {\cdot}0012 \times 10^{-6}$.

As regards the rapid drop in the value of the coefficient which follows the rapid rise at the annealing temperature, the values in this region may be subject to considerable error, as, when this stage is reached, the specimen is soft enough

* *Journ. Scient. Instr.* 1 (April, 1924) 198.

for slow flow to take place under the influence of its own weight, and the measurements of expansion become quite meaningless. Only a volumetric method could give reliable measurements of thermal expansion about the softening point. I do not know if anyone has overcome the obvious practical difficulties of such a method, so as to establish what really happens to the coefficient above the annealing temperature. My own interest in the matter was temporary and I have not followed the literature. I think all that can safely be said as the result of measurements of linear expansion is that a very rapid and large rise in the coefficient of expansion takes place at the annealing temperature. The exact extent of this rise, or what happens as the temperature is raised still further, cannot easily, if at all, be measured except by a volumetric measurement.

Mr T. Smith: It is somewhat uncertain whether Adams and Williamson's results may legitimately be analysed further than Mr Hampton has done in this paper; if this is not permissible the type of expression given in equation (32) is a satisfactory representation of the observations. It is then tempting to insert the Twyman temperature function—or whatever other function is found to fit the observations—as the value of f_L, multiplied of course by a constant. Should this prove on fuller investigation to be justified the whole theory will assume a simple form. It has to be noted that the rise of temperature which doubles the rate of release of strain is about 8° C., whereas the figures for f_L at the bottom of p. 175 suggest about 20° C. It is however probable that the value at 385° is subject to considerable uncertainty, and the observations so far available can hardly be regarded as definitely contradicting this supposition.

Mr Hampton: The change in α between 0° and 300° C. to which Mr Guild refers is only of the order of 15 per cent. The change which occurs in the region of θ_0 is of the order of 200 to 700 per cent. Also the value of α, which is taken as unity, is that value corresponding to the temperature range of about $\theta_0 - 100$ to θ_0. Hence to a sufficient degree of accuracy α may be assumed as a constant.

The second point was investigated in some detail by Peters and Cragoe*. They proved by measuring the horizontal expansion of rods that the contraction was not caused by the glass flowing under its own weight. They did, however, find a permanent contraction in their specimens which they attributed to spheroiding by surface tension. This invalidates, as Mr Guild points out, the values of α above the point of rapid increase. Fortunately reference to Tables VIII and IX shows that the value of $\frac{s_0 - s}{s_0}$ has already fallen to ·05 before a reduced value for α is made use of. Hence uncertainty in α will not seriously affect the conclusions drawn in the paper.

I am glad that I have been able to remove some of the points of difference between Mr Smith and myself by this later work. It is not immediately apparent why f_L should follow Twyman's law but the uncertainty in the available data is such that the question must be left open for the present.

* Peters and Cragoe, *Journ. Opt. Soc. Amer.* 4 (1920) 140.

THE NATURE OF THE POLISHING OPERATION

By F. W. PRESTON

[Communicated by Mr F. Twyman, F.R.S.]

MS. received, 14th December, 1925. Read and discussed, 11th February, 1926.

ABSTRACT. The status of the "flowed" layer on glass, quartz, and similar materials which have been ground or polished is unsatisfactory. The present paper calls attention to a number of phenomena that seem to bear on this subject, and might serve as starting points for interesting researches. It is believed that interesting relevant observations have been made by a number of people, who could contribute to a symposium, and by so doing make clear how much agreement or disagreement there is on the subject, and so prepare the field for future investigators.

Introduction

When the late Lord Rayleigh first interested himself in the nature of the process of polishing glass(1), he seems to have come to the conclusion that the polishing operation was probably entirely different from the grinding operation. A ground surface (on glass or quartz) was produced by fracturing: a polished one by flowing, or a "molecular or nearly-molecular" process.

The late Sir George Beilby showed(2) the existence of a flowed, disturbed, or amorphous layer on most polished bodies, including some very brittle ones. He appears to have held views very similar to Lord Rayleigh's, but seems inclined to have thought there was some "flow" even in grinding. This partially abolishes the sharp distinction between the two processes. Unfortunately, Sir George Beilby's work on glass and quartz was not very extensive, forming an insignificant part of his investigations, for his main interests lay elsewhere. Some of his conclusions in respect of these particular substances are almost certainly the result of misinterpretations of his photographs. Figs. 76 and 77, for example, prepared by Mr Haigh, are described by Sir George Beilby on p. 111 as "revealing the original grooves and pits which had been flowed over during the polishing operation"; actually the structures revealed are chatter-sleeks(3), which are not grooves at all, but are a product of the polishing operation itself, not part of the "original" structure.

The present writer had the privilege of discussing these matters with Sir George Beilby a year or two before his death, and agreed on most of them. Sir George inclined to an estimate of one or two millionths of an inch (25–50 mμ) as a probable maximum depth for the flowed layer on glass. The flowed layer on calcite he estimated (p. 99) as averaging perhaps 500 mμ (20 millionths of an inch).

On the other hand Dr French(4) has interpreted some of his results as indicating a flowed layer on glass of a vastly greater thickness, viz. about 8 wave-lengths

(about 150 millionths of an inch). The experiment on which this conclusion is based is certainly ingenious, but not conclusive. (Unfortunately Dr French has used it also to arrive at quite inadmissible further conclusions(5).)

However, Dr French has also shown(4) that the flow on glass is much less than on some other materials: he failed to persuade glass to cross the most minute cavity of an air-bell, unlike Sir George Beilby, who caused copper to flow across cavities (Figs. 54–57, p. 90) in films estimated at from 10–20 mμ thickness (less than a millionth of an inch). This observation of Dr French's is very much against the view that the surface layer of glass is flowed to a depth of 8λ in polishing.

The present writer has given reasons(3) for believing that any flowed layer there may be on glass must be very thin, not exceeding a millionth of an inch or so, and that whatever influence "flowing" may have in the polishing operation, it is by no means the whole story; that in fact the rôle of "flow," or of surface tension effects, seems to have been rather exaggerated, and that the process of polishing is primarily one of mechanical abrasion (as the older writers used to think), with possibly some flow taking place as a secondary effect. This seems also to be Mr Twyman's view.

Unfortunately, it seems difficult to prove that there is any such flowed layer at all on glass, and to prove it unequivocally. If there is such a layer it is a thin one, but its very existence seems uncertain. The same is true of polished quartz, where the chances of detecting an amorphous layer are better.

Recently Ray has shown(6) that when silver sand (quartz) grains are *ground* for a time they decrease in density and change in heat of solution. This is most readily interpreted as meaning that some crystalline quartz has changed to amorphous silica. But unfortunately this experiment does not help us in deciding the nature of the polishing operation, for it seems to mean that if we *grind* the surface of a piece of quartz, the surface layer thereof is disarranged and becomes less dense and more soluble. And these are precisely the properties found by myself in the *ground* surfaces of glass, but less conspicuously in polished surfaces. Hence, whatever Ray's results do show, they do not prove that there is any radical difference between ground and polished surfaces, as postulated by Rayleigh, but rather the contrary. In other words, the presence of a disarranged surface layer is a natural concomitant of mechanical working of that surface: it is not peculiar to "polished" surfaces, but is better developed on typical rough-ground ones. (A polished surface may then, for the present purpose, be regarded as a very finely ground one.) It must be admitted that this view is a logical one, and it seems to explain most of the phenomena, but it is getting a long way from Lord Rayleigh's original notion.

Thus, the following alternatives seem to be available:

(*a*) Ground and polished glass surfaces are essentially similar, the products of mechanical abrasion. The glass at the surface is similar to the massive glass below (Herschel, etc.).

(*b*) Polished surfaces are radically different from ground ones, the former having been, as it were, liquefied or rendered mobile and then moulded by surface-

tension forces rather than by mechanical abrasion (fracturing) (Rayleigh, Beilby).

(*c*) The surface layers of ground and polished surfaces are essentially similar to one another, but differ from the massive glass below. The difference is the product of mechanical abrasion rather than of "flow" as ordinarily understood (Ray, Preston). In such a case the disturbed layer is thicker on ground than on polished surfaces.

Obviously variants of these three views are possible.

The present paper does not aim so much at settling the question as at finding out what the question is, and at indicating lines of attack on the problem. Some of the present confusion may arise from the fact that our terms have not been sufficiently defined. I discover, for example, that there is no clear agreement as to what is meant by surface flow. Some observers regard it as a process of smearing the surface glass from one place to another, very similar to the operation of buttering bread. Others think that the mobile molecules are not merely slid about on the surface like this, but may be actually detached entirely from the surface and then re-attached elsewhere. Others again believe that there is little transport of material at all, that the process rather resembles the operation of ironing clothes. Others, again, do not believe in "flow" at all (7). The layman seems to interpret "flow" in the bread-and-butter fashion, which seems nearest to Sir George Beilby's original views, and the only way in which skins may be flowed across cavities as found by Beilby for copper. Most of the workers in this field, however, seem now inclined towards a less drastic sort of "flow."

By "mechanical abrasion" we mean as a rule the process of breaking, chipping, or flawing away of particles from the surface by the action of minute abrading grains or tools. The particles are entirely detached in the process. If they are re-attached later, that does not constitute "flow."

Relevant Phenomena

The phenomena that have come to my notice and seem relevant to the subject will now be briefly summarised under the following heads:

(1) *Abrasives*. The abrasives used in polishing seem to be characterised solely by their being particularly hard and insoluble; they are such as are unlikely to exert or produce any chemical action. Such are the oxides of iron, tin, chromium, aluminium, bismuth, etc., and a few other substances, e.g. silicon carbide, according to Dr French.

Further, these abrasives do not increase in efficiency with increasing smallness, i.e. do not act in virtue of their surface energy. Sir Herbert Jackson tried to make a polishing powder by collecting the dust from the iron arc; he thus obtained an exceedingly finely divided material, but one utterly useless as a polishing agent.

Most of the oxides listed above are characterised by the fact that they can be prepared "soft" at low temperatures, and increase in density, hardness, insolubility, and general refractoriness on firing at higher temperatures, and it is the high temperature product alone that is useful for polishing.

Thus a study of polishing powders suggests that they act simply by their ability to concentrate great pressures on small areas and so, presumably, to crush and fracture the glass surface.

(2) *Fluorescence*. Some substances are fluorescent in the crystalline condition, and not in the amorphous one. Sir Herbert Jackson suggested that the presence of an amorphous layer on a crystalline body might thereby be demonstrated.

(3) *Flowed Layer*. Sir George Beilby reported that the flowed layer on calcite was vastly tougher than the crystalline material below. "After a surface skin has been fully developed by polishing, it is able to resist the cutting action of the powders by which the surface had been satisfactorily ground at an earlier stage" ((2), p. 101).

Nothing of the kind is observed with glass. By actually weighing the loss of glass at regular intervals, and using controlled conditions of polishing, I find no signs whatever of a progressive toughening of the glass. Material comes off steadily at the same rate, whether the glass is quite "grey" ((3), p. 150, footnote) or has been polished for many hours.

The study of the rate of glass removal indicates that the flowed film, if it exists at all, has no direct influence on the process. Mechanical abrasion—i.e. the complete removal of glass—is the conspicuous feature.

(4) *Twyman Effect*. When a piece of optical glass is ground on the faces it produces a double refraction (Twyman effect) throughout the body of the glass, the surfaces being in intense compression and the body of the glass in tension (3), (8), (9). The double refraction disappears when the surfaces are polished. This suggests a true qualitative difference between ground and polished surfaces. But Dalladay has shown that the surface-compression varies with the coarseness of the abrasive used and for fine abrasives is much less than for coarse ones. Now the abrasives used in polishing are very fine ones and the rate of removal of glass in polishing is only about 1/50 that in fine grinding. Dalladay's results suggest that the double refraction should then be perhaps only 1/50 that observed in fine-ground specimens, and this would render it virtually undetectable.

Sir Herbert Jackson in conversation with the writer expressed the opinion that this slight surface compression did in fact exist, but it was difficult to get a sufficiently large, perfect, piece of optical glass to make sure. There is here a hiatus in our knowledge. The denotation of a measurable surface compression would be a heavy blow to the "flow" theory, which would seem to call for a *tension* in the surface, exceeding the surface-tension normal and inherent in any free glass-air surface, such as a "fire-glazed" surface.

It is entirely possible that the surface tension that must exist in any piece of glass, and whose amount has been estimated by Griffith (10) and others (11), acting in opposition to the surface compression due to polishing, as postulated by Sir Herbert, might result in a body stress actually less than is obtainable with a fire-glazed specimen. For it is obvious that the surface tensions of an ordinary slab of glass must produce a body-compression, i.e. the material at a depth below the surface must be in compression.

There is room for a careful investigation as to whether any Twyman effect can in fact be detected in well annealed, polished, glass; and if so, what its sign is.

(5) *Effect of Etching.* The present writer has shown (3) that, when a polished glass or quartz surface is lightly etched with hydrofluoric acid, detail of the order of 10^{-5} of an inch can be made out, and there is every reason to suppose that if microscopes possessed greater resolving power than this, detail of a still finer order might be discovered. The detail consists principally of "chatter-flaws" ((3), Fig. 9, p. 144), which could not exist if the flowed layer were more than a few millionths of an inch thick. These tiny flaws seem to be right close up against the surface, not in some lower layer, for the amount of glass dissolved away by the acid in etching up the structure is extremely little. On this point more work might well be done, since, if these flaws really come right up to the surface, it is apparent that there can be no "flowed layer," in the ordinary sense of the word, at all. And if on the other hand there is a flowed layer covering these tiny fissures, then it is evident that the flowed layer must be entirely dissolved away before the acid can etch them out and render them visible. The amount of glass actually removed can be determined by weighing, and an upper limit for the thickness of the flowed layer thereby obtained. Some preliminary measurements were made on this point by the present writer and recorded in the paper referred to. After allowing for the fact that *some*, at any rate, of the loss of weight must represent glass removed from the flaws themselves and is therefore "massive" glass, not flowed glass, it is apparent that the amount left for the flowed layer is very slight. More accurate observation might show that the flaw structure becomes apparent with much less etching than I recorded: this is largely a matter of microscope technique.

(6) *Chatter-sleeks.* When a piece of polished glass is re-ground with coarse abrasives, the first few passes of the abrading grains frequently produce chatter-sleeks; later on, when the whole surface is broken up, nothing but disconnected percussion flaws ((3), Figs. 16 and 17) are produced. Thus it might seem that when the irregularities of the surface are small compared with the size of the abrading grain we may get chatter-sleeks, but not when the irregularities are large; in other words, the presence of chatter-sleeks may be an indication that the general irregularities, other than the flaws of the sleeks themselves, are extremely fine in comparison. But chatter-sleeks are the characteristic feature of polished surfaces, as the other irregularities of polished surfaces must be, for the most part, a great deal less than 10^{-5} inch in size. This seems to me a point rather in favour of the existence of a thin flowed layer, for the great regularity of the flaws in the finer chatter-sleeks suggests that any other irregularities must be of virtually molecular dimensions. The point, however, is not certain, since other reasons may be adduced to account for these features, as shown below.

(7) *Effect of Load.* As I have previously shown (3), it requires a certain minimum load upon an abrading grain to produce a hyperboloidal flaw or a chatter-sleek, and there is a corresponding minimum diameter of this flaw and a minimum width of such a sleek. I have, however, drawn lines on polished glass with spherical steel and diamond points, of about half a thousandth of an inch radius, with

pressures less than the critical. Undoubted marks are produced, which are enhanced by etching with hydrofluoric acid; the lines are usually multiple and their structure is very obscure. As a rule, little or no double refraction is produced in their neighbourhood, and the phenomena rather suggest that some surface disturbance has taken place which might conceivably be regarded as "flow," though where the flow is to or from is not obvious.

(8) *Dimensions of Flaws*. The widths of chatter-sleeks or the diameters of hyperboloidal flaws ought, according to the Principle of Similitude, to be proportional to the diameter of the grain producing them, but they are not. The smaller grains produce disproportionately wide sleeks. If the laws discovered by Auerbach (12) and the present writer can be extrapolated for grains as small as those of polishing powders, the diameter of the flaw or sleek will be approximately the same as the diameter of the grain, or possibly greater.

But in the loaded surface of a polisher the rouge grains are packed together like a cobble-stone pavement, and the flaws produced by one grain will lie in the territory of its neighbour. This interference of the activities of one grain with another must produce severe complications: where one grain tends to produce a tension, its neighbour produces a compression; this may account for a number of phenomena—for example, the extraordinary inefficiency of the glass-polishing operation, which is only a fraction of 1 per cent. In fact it may be that it is this interference of one grain with another that is the true characteristic of the polishing operation—the feature that really distinguishes it from the grinding processes.

This point could do with further investigation.

(9) *Temperature Effects*. Abrasive wheels produce, as a rule, unmistakable signs of flow, but this is due to their getting very hot; even under water they can frequently be seen to glow bright red at the contact, and glass that is removed in fragments is frequently welded into a coarse turning or string. The appearances of the surfaces produced are as a rule quite distinct from those of polished ones, and the structure developed by etching is also different. The matter is interesting, however, as indicating the variety of structures that may exist below the surface layer.

A diamond ruling may also show signs of flow, but here also the temperature is probably high at the moment of production.

Glass that has been polished on felt with a supply of rouge, but without water, and under considerable pressure, shows a peculiar amalgamation of rouge and glass, and an appearance strongly suggestive of actual flow. It is interesting to observe, however, that the stronger the suggestion of flow, the less satisfactory is the polish. In fact the tenor of these experiments is to show that the technique of successful polishing consists principally in avoiding "flow."

(10) *Refractive Index Tests*. Mr Twyman has shown that the refractive index of the surface layers of glass and quartz decreases with time, an effect ascribed, I believe, to hydrolysis. Mr Lee has made a very careful search to see if any difference of refractive index could be found between a newly polished surface

and the massive material below; the results appear to have been entirely negative. Thus the flowed layer either does not exist, or else it is very thin, or else it has the same refractive index as the massive material. The last alternative is inconceivable in the case of quartz.

(11) *Interfacial Tension Tests.* The manner in which water spreads, or fails to spread, on glass surfaces is very interesting. Water spreads readily on a *ground* glass surface that has been chemically cleaned*, but it does not spread readily on fire-glazed or polished surfaces. It spreads very rapidly on *any* surface that has been lightly etched with hydrofluoric acid solution, and, of course, dried again in an air current. This might be explained if polished and fire-glazed surfaces had a thin β film†, while ground glass had not. But there is no obvious reason why the fire-glazed surface should have just a film of β; it ought to be β all through.

Again, experiments were made with the natural faces, both prism and pinacoid, of a very fine crystal of quartz which seemed to have been very carefully preserved from abrasion of any sort. It was found that here again water did not spread readily even after very careful cleaning, but an insignificant etching with hydrofluoric acid resulted in the water spreading with great speed. There was no visual evidence that the crystal had ever been touched by acid, but the boundary of the etched part was very distinctly located by the change in the behaviour of a spot of water allowed to flow across it.

Since there can be no question that these crystal faces possessed no flowed layer, the change produced by etching cannot be ascribed to the removal of a flowed layer. The spreading of water on fire-glazed and mechanically polished surfaces follows exactly the same rules as apply to natural crystal faces of quartz, and therefore these experiments, like most of the others, seem to give a negative result in our attempt to demonstrate the existence of a flowed layer.

(12) *Electrostatic Experiments.* It has been known for many years that, on rubbing with silk or wool, polished glass surfaces (including fire-glazed ones) become positively charged, while "grey" (ground) ones become negative. Partly polished surfaces behave like completely polished ones, even when the amount of polishing is extremely slight, but on etching with hydrofluoric acid solution they become electrically, as well as visually, "grey" again.

Completely polished surfaces do not become electrically grey on etching. These results therefore would seem to indicate that "electrical greyness" is merely an indication of roughness.

But it was frequently found that "fire-glazed" surfaces did become electrically grey on etching. This result seems difficult to square with any theory whatever. The results of electrostatic tests, in fact, seem very complex and vary with all sorts of conditions. This phase of the subject seems to be a complete chaos at present.

(13) *X-ray Methods.* Experiments on the reflection of X-rays from crystal faces and polished faces should, one would think, afford conclusive evidence of

* I.e. with distilled water and alcohol.

† J. W. French's name for the "flowed" glass.

the existence or non-existence of a flowed layer on polished faces of quartz. Sir William Bragg, in a letter to the present writer, states that it is sometimes convenient to *roughen a crystal face* on a grinding wheel before testing it, but that two diametrically opposite interpretations may be placed on this fact. So far as is known, no experiments have yet been made on mechanically polished surfaces, which therefore offer a virgin field to the investigator. If an amorphous layer is found, it should be possible to determine its thickness by etching it away and weighing the loss.

(14) "*Enamels*." In the ordinary process of polishing as practised in the optical and spectacle industries, there are sometimes produced what are usually called in England "enamels" or in America "burns." The English term is preferable, since the term "burn" is often used, even in America, in other senses. An enamel commonly takes the form of an incrustation of rouge on the glass surface; it cannot be removed by rubbing with the hand, even with the aid of water and much pressure, and is usually caused by running the polisher too dry and with too much rouge. Fine rouges are usually more troublesome than coarse ones in this respect; the polisher may be pitch, wax, or felt. The only practicable way of removing an enamel is to go on polishing, but using more water and less rouge.

However, an enamel is not necessarily an incrustation of rouge; rouge may be entirely absent, and is in all cases a secondary feature. The fundamental substance of an enamel is an entirely different material.

The outlines of an enamel are usually irregular, but in polishing a single disc, beautifully regular ones are sometimes produced. Interference tests (using the ordinary "contact gauge") show that the enamel is *always* raised above the proper surface of the glass. And what is more, and very striking, there is apparently always an invisible enamel extending beyond the edge of the visible one. This invisible enamel is 3 millionths of an inch thick ($1/6\lambda$) and has a sharply defined margin, and also steps up about 3 millionths of an inch. This layer shows whitish by reflected light. The next layer again, usually with a margin a trifle less distinct, steps up a somewhat similar amount, and shows yellow; we then get a blue ring, and so on up the Newton scale of interference colours. If the enamel gets any thicker than this, rouge usually runs into it and hides the phenomena a good deal.

Sometimes I have found enamels consisting solely of the invisible primary layer, about $2\text{–}3 \times 10^{-6}$ inch thick, and only discovered at all by the interference test.

Chemical and micro-chemical tests show that the material of an enamel is insoluble in all organic and most inorganic solvents. In most cases it can only be removed by hydrofluoric acid, which attacks it very readily. Hence there is good reason to suppose that the fundamental substance of the enamel is glass.

The definite steps of the enamel, all of about the same height, show that the substance must be in discrete particles about 3 millionths of an inch in diameter; and it would seem strong evidence that the glass is removed in polishing in particles of about this size, and that sometimes, if the glass gets too hot, they are stuck back again on the polished glass surface. Usually they are stuck down in

irregular masses, but sometimes, as above indicated, in very regular ones, which are the best subjects for study.

The question then arises, By what means are these particles re-attached? Are they fused on? It is a question that the writer is at present unable to answer, but the phenomena of enamelling seem to show that the general process of polishing consists in the removal of particles of a definite size, well above molecular limits, i.e. that the process is not a molecular or nearly-molecular one as suggested by Lord Rayleigh (1), but distinctly a molar one.

Conclusions

The position may be summed up as follows. During the process of glass polishing, glass is continually removed, and the rate of removal does not slow down with time. Coarse rouge is better than fine for polishing, provided it is not so coarse as to produce sleeks visible without etching. Any clear signs of flow or fusion represent defects of polishing, but the mere fact that such phenomena do seem to occur, if the temperature is allowed to rise somewhat, suggests that "flow" or fusion of some sort on a molecular scale may in fact be operative simultaneously with the more important phenomena of mechanical abrasion. The evidence for the existence of a surface layer on polished glass, differing from the underlying material and from the surface layers of ground, or fire-glazed, or crystalline surface layers in the case of quartz, is slight or negative. Whether we test for interfacial tension, solubility, electric properties, refractive index, or microscopic properties, we seem to find no satisfactory evidence of any distinctive flowed layer, while a study of the rates of abrasion, properties of abrasives used, "enamels," and so on suggests that the process is principally one of ultra-microscopic abrasion. A study of the failure of the principle of similitude in the formation of flaws by grains of different sizes suggests that the process of polishing may be a process of abrasion complicated by the fact that the fissures produced by one grain lie in the territory of its neighbour, and that the abrading efforts of the several grains tend to neutralise one another.

The subject seems to be clearly one for further investigation, and as the present writer's facilities for such investigation are small, the present paper is written in the hope of inducing others to attempt it.

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DISCUSSION

Mr J. Rheinberg: A certain amount of information regarding the nature of the surface layer of polished glass can be gleaned from experiments I conducted some years ago in introducing metals—platinum, palladium, iridium, and gold—into this layer. The glass was coated with collodion containing appropriate salts of the metal, and then burnt into the surface layer in an electric furnace* at temperatures varying in practice between 600° and 700° C. That the surface layer did differ from the glass below was amply proved by the fact that its viscosity became sufficient to allow the metal to penetrate at 5° to 10° C., according to the constitution of the glass, below that at which it would penetrate the mass of the glass. Unworked sheet and plate glass required a higher temperature for the metal to penetrate than if the same kind of glass had been worked. If the temperature were too high, so that the metal penetrated below the surface layer, that could be recognised by differences in the appearance of the platinised glass as well as from other indications. From analogy with experiments of Beilby on thin metal films I estimated at the time that the thickness of the surface layer was of the order of 1/3 wave-length of red light, i.e. about 6 or 7 millionths of an inch. In some experiments with worked quartz, whereas I expected that the requisite temperature would considerably exceed that of glass, this was in practice found not to be the case. Whilst these results excluded the conclusion that the glass at the surface is similar to that of the massive glass below, and I incline to the conclusions of Beilby and Rayleigh on the subject rather than to those of the author of the present paper, the matter is of course not one on which any further definite opinion can be expressed off-hand.

* "Platinum Reflecting Surfaces Prepared by the 'Burning-in' Process," published in a discussion on "The Making of Reflecting Surfaces" by the Physical Society of London and the Optical Society (November, 1920) p. 28.

NOTES ON THE SIGNIFICANCE AND DETECTION OF LOW ERRORS OF REFRACTION

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ABSTRACT. Examination of a great number of records of tests on the eye for the purpose of prescribing spectacles reveals the large proportion of low errors of refraction. The author suggests certain modifications of the usual technique that, in his opinion, should be incorporated when testing such cases.

I. Importance of Careful Testing of Low Refractive Errors

More and more, as time goes on, the modern refractionist is compelled to pay increasing attention to the estimation of low degrees of refractive error of the eye. At one time it was thought that any ametropia of less than one dioptre might be neglected, just as in the very early days of microscopy it was considered that a sliding-tube adjustment for a microscope was usually sufficient.

The wider recognition of the fact that many disorders of the nervous system arise from the unconscious effort to correct small refractive defects has necessitated an entirely different outlook and technique. In a recent survey of nearly 100,000 records I was struck by the very frequent occurrence of corrections under one dioptre. The actual statistics are not yet available, but I hope, if circumstances allow, to publish at a later date comprehensive tables of the incidence of such errors, together with the corresponding symptoms complained of by patients seeking optical relief. The task of classifying such a large number of records, even under only a few headings, is a lengthy one and, if the grouping is to be of maximum benefit to those interested in ophthalmic statistics, it becomes formidable. In any case it would be of great assistance to have the views of members of this Society as to the headings under which they consider information is desirable; partly with that end in view it is my intention to bring forward at this stage some suggestions concerning technique that seem to me, from a general survey of the records, to be necessary in the consideration of these small errors. It is hoped that from the discussion will emerge information of service in carrying out the classification.

Technique is a word which has different meanings for different people. It will be used here in its widest sense, that is, to include every factor which may have any bearing on the operation and its success or failure to accomplish a desired end. Taking the term in this sense, the records point to the supreme necessity for the refractionist to believe fully in the efficacy of correcting even the smallest deviation

from the normal wherever symptoms have been sufficiently obvious as to have aroused a complaint from the patient.

There does not appear to be a very wide and authoritative literature on the subject. Various writers, both medical and optical, have mentioned symptoms which may accompany these errors and quoted clinical cases in support, but in all these accounts there is a certain reserve of statement which tends to create the subconscious idea that such cases are comparatively rare, or that seldom are the symptoms so severe as to warrant a regular routine examination for low errors of refraction in every patient presented for report. Even at the present day it is extremely common for patients to remark that they have been previously tested and told that their sight is "practically perfect" and they do not require glasses, when careful re-testing has afterwards revealed the fact that low errors are manifested, the proper correction of which has at once relieved the symptoms complained of.

It still more frequently happens that patients present themselves who have been provided with glasses of low power but which they have been unable to wear with comfort, the powers of the lenses and their positioning before the eyes not having been determined accurately enough. When rightly corrected the symptoms have disappeared and the glasses have not only been borne with comfort but considered as a first necessity.

Considerations of the above facts indicate the essential need for a faith in the value of careful and painstaking examination of every patient whose visual acuity may, at first sight, appear to be normal. The recognition of the further fact, which no doubt the statistics previously mentioned will eventually show—namely, that far from being rare, cases of headache, nervous fatigue, irritability, and other obscure neuroses which arise from the muscular efforts to overcome low errors, are extremely common—should stimulate the refractionist to further effort in research work in this direction.

II. Suggestions as to Technique

(*a*) *Priority of Subjective Test*

Granted that the patience borne of real faith is possessed, the next desideratum is an absolute command of every possible resource in subjective methods of testing. Apart from certain factors which will be dealt with at a later stage, I would submit that the usual objective methods of sight examination are practically useless in the estimation of errors under one dioptre, in spite of the claims of some refractionists that, by retinoscopy, errors as low as a quarter or even an eighth of a dioptre can be estimated. It will be admitted that there are cases where the psychological result of certainty of success on the part of such refractionists induces a corresponding certainty of cure on the part of some patients; nevertheless I am convinced that a subsequent referendum of patients so tested will show that in a large proportion of such cases the relief has only been transient and that many have discarded the correction so obtained.

The recommendation of subjective methods as the only ones suitable for these low refractive cases does not, however, mean a weak reliance on the patient for the result. On the contrary, it implies a close cross-examination conducted in such a subtle manner that the patient is unaware that any reliance is placed on his opinion or indeed that his opinion has even been asked. It is usually assumed by students of refraction that subjective examination is a method of purely oral "question and answer" in order to find out what a patient sees. In the preliminary rough and ready methods used to obtain an approximate or, as it is usually styled, a "6/6" result, this is largely true. In the special cases before us, however, it is to be remembered that the patient with low error can usually see 6/6 to commence with and it is not so much desirable to find out merely what he sees as how he sees. Furthermore, the question of "how he sees" involves the very elusive problem of "how he feels" when seeing, and it is this factor of feeling which is of paramount importance in the successful treatment of these cases.

It may be advanced that such teaching is heterodox, but long experience has taught me that the subjective examination should precede the objective examination of the eye and that, where recourse has to be made to the retinoscope or ophthalmoscope prior to the subjective test, such glances should be of the briefest—almost momentary—duration. The reason advanced for a full preliminary examination by objective methods prior to sight testing is that such a close external and internal examination is necessary to ascertain the presence of any pathological lesion. I believe this procedure to be entirely wrong and for the following reasons. The large proportion, at least 90 per cent., of cases presented to the refractionist are cases of purely refractive or muscular error. Of this large proportion, the majority are of low degree, 50 per cent. at least having vision 6/9 or more and a large proportion of these 6/6. Now anyone who has been subjected to a close preliminary objective examination knows that the unaccustomed glare produces distinct after-images, physical discomfort, and an altogether artificial mental condition.

If my contention be true, that the correct estimation of low errors depends on a careful examination of what the patient feels as well as what the patient sees, it is obvious that, when a prolonged objective examination is made, the normal and characteristic physical and mental poise has been artificially upset to commence with. To use an illustration taken from another profession, let us consider for a moment the difference between the technique of weighing a substance on an ordinary pair of scales and precision weighing on a chemical balance. In the former case we have two scale pans and a series of weights and we estimate the weight on one pan by placing counterweights on the other side until the pans are approximately level. In the chemical balance we weigh under entirely different conditions. In order to prevent the influence of draughts the balance is entirely enclosed in its glass-sided case. To prevent rust and dirt on the weights we use forceps. For fine weighing we use the principle of the lever by adding a rider, and to get still finer adjustment we read the deflections on a graduated scale from a long arm. Lastly, to facilitate speed in work, we have an outside screw adjustment for steadying the pans.

Now coming back to our work as refractionists, it is submitted that, as most of

the cases we have to estimate are of low error, we should design our technique to ensure a precision comparable with that of the chemical balance rather than that of the rough and ready scales. To pursue the illustration, it has always appeared to me that the preliminary objective examination sets up an unnatural balance from the start. Not only have a number of after-images on and around the macular area been created which perceptibly interfere with vision, but the added artificial conditions make it as difficult for the patient's normal feelings to be ascertained as it would be difficult for the analyst to attempt to weigh a heated substance in a draughty position.

(*b*) *Priority of Binocular Test*

The immediate operation indicated then—having ascertained by a rapid preliminary retinoscope estimation that the correction is low and that vision is fairly normal—is to proceed with the subjective test. The *modus operandi* suggested is still more likely to raise the cry of unorthodoxy; it is to add convex spheres *binocularly*—before testing the eyes separately—first using one dioptre and, if necessary, further additions of a half dioptre or less, asking at each stage if the line first seen can still be made out; to use the exact phrase, "Can you still make out that line?" The success of eliminating accommodation by this method depends entirely on the expression of the question; it must be asked without emphasis and almost in a disinterested manner. If there is any undue emphasis on the word "still," or if there is any shade of doubt or anxiety expressed, the patient will almost invariably respond to the feeling expressed or, what is worse, negate the question with either scorn or emphasis. It may be said that it is highly undesirable to lay before a scientific society so unscientific a method of testing as one which relies on mere feeling or the nuances of the voice. I do not think such criticism has any real basis. It is to be understood that I am presupposing that the refractionist has such clear, scientific knowledge of the optical system he is estimating by the patient's answers that he is able mentally to visualise exactly the possible varying appearance which the patient is experiencing. The added recognition of the fact that induced sympathy or induced distrust will materially alter the patient's reply, creates the necessity for the refractionist scientifically to train and control his own method of speech in order that he may correctly and scientifically interpret the patient's replies.

These more general reflections are interposed at this stage in order that the actual principle underlying the suggested technique may be better understood. For instance, fogging to any extent greater than two or three lines beyond the original is not only unnecessary but is usually fatal to continued confidence on the part of the patient. Further, the question should not be asked at all if the patient's facial expression has already answered it. Instead the question should be altered to "Can you now make out any of the letters on *this* line?" pointing at the same time to the second or third line above, according to the indication expressed facially as aforesaid. In brief, an essential part of the art of subjective testing consists in watching the facial expression rather than relying on the actual oral reply. It is the face which in nine cases out of ten gives the correct answers if the questioning is sufficiently unbiassed.

In those much rarer cases where the face remains impassive, the oral replies may be accepted as literally correct.

The reason for adding spheres binocularly instead of monocularly is twofold. Firstly, the blocking of one eye is usually subconsciously, if not consciously, resented and strain is thereby induced. Secondly, relaxation of accommodation is much easier with the two eyes working in unison, as slight blurrings due to astigmatism or over-correction are partly remedied by each eye.

(*c*) *Further Conditions to be observed*

1. Another important point in technique is to ensure that the change of lenses is made smoothly and without allowing the eyes either to be uncovered by a lens or to be covered by two lenses. That is to say, the lens substituted must be inserted before the pupil as the other is withdrawn. Otherwise the patient may say, and often will say, that lens B is better than lens A when it is actually worse because he has really compared lens B with lenses A and B which were left momentarily in the frame together. Many wrongly diagnosed cases of temporary spasm of the accommodation are due to this simple mistake of technique.

Errors of astigmatism which are frequently missed through the patient saying that all the lines are alike are discovered by a method which Dr Clay described in a paper* read before this Society in 1915 and which he described as the "oval peg in the round hole" method. That is to say, weak cylinders are rotated in the opposite meridians thus doubling and correcting alternatively the defect, a method which rarely fails with even the least intelligent patient. The use of a Jackson crossed cylinder has so many advantages and is so widely known that I only need mention it.

2. Another most necessary procedure in the right estimation of low errors is to under-correct as well as to over-correct whilst testing. Most students of refraction are urged to give plus and avoid minus, the old habit remaining long after practitioner days have commenced and being eradicated only as the result of long and painful experience. The fact is that it is just as necessary in the routine to put on extra minus as it is to put on extra plus when estimating low corrections. To use our analogy of weighing once more, it is necessary to observe the extent of the swing of the pointer arm on both sides of the zero of the graduated scale on a chemical balance when fine weighing, and similarly in sight-testing the contentment expressed on one side of the approach to zero must be measured against the mere tolerance felt by a similar approach on the other side.

It appears to me that possibly one reason that so many plus 0·5 cylinders are prescribed instead of minus 0·25 sphere with plus 0·5 cylinder is due to the fact I have just mentioned. A little reflection will show that so far as nature is concerned there is no more reason for hyper-astigmatism occurring more frequently than mixed astigmatism; indeed, if anything, the crossed cylinder is more nearly normal. It will be frequently found that the extra minus 0·25 sphere is the factor which relieves the patient from the feeling of oppressiveness when wearing a weak

* R. S. Clay, *Trans. Opt. Soc.* 15 (1914–15) 99.

cylindrical correction. Here again the facial expression of the patient is the only dependable factor. The necessary minus will give an instantaneous manifestation of contentment or repose, whilst further minus is merely tolerated.

3. Thus far I have dealt but briefly and perhaps somewhat sketchily with the strength of spherical and cylindrical elements. Another factor quite as important, so far as full relief of symptoms is concerned, is the direction of the axis of the cylinder. Many cases have come under my notice where more careful adjustment of the axis and an alteration of 5 or 10 degrees on a low power has made all the difference between comfort and discomfort. For these fine adjustments confusion letters and 6/4·5 and even 6/3 type are invaluable.

4. The question of fine adjustment in muscle testing deserves a paper almost to itself. In my own case, the full correction of minus 1·25 sphere is unbearable unless worn with a decentration of 5 mm. up and in on one eye, a correction which does not seem to have any relation whatever to the actual inbalance of 10- or 12-prism dioptres of exophoria and an almost negligible hyperphoria as found by the ordinary methods and the usually accepted rules for calculating the amount of prism necessary.

There are, however, two observations I would make. Firstly, with regard to the distance test for hyperphoria, it does appear to be most necessary to carry it out under very carefully prepared conditions specially designed to allow one to eliminate artificial hyperphoria caused by the wrong position of the head or lenses, and also to enable one to estimate accurately the presence of such low degrees as half a prism dioptre, as the incorporation of low prisms frequently relieves distressing headaches and other symptoms. Secondly, low muscular correction can, with an intelligent patient, be frequently subjectively corrected by allowing the patient to hold the correction in front of the eyes and circling the lenses upwards and downwards and inwards and outwards until "the most comfortable" decentration is arrived at. This method of course is only possible where there is sufficient spherical component to provide the necessary variation of prismatic element, but when it can be used it is an invaluable extra refinement of the test.

5. Finally, there is one disturbing factor in the estimation of low errors of refraction which is very commonly overlooked by many even experienced refractionists, that is, the presence of slight irregular astigmatism which is not by any means rare. It is, on the contrary, very common indeed and accounts for the remarkable variations which take place in successive tests on the same patient. In these cases again, it is comfort that has to be sought, and many of the unorthodox methods I have suggested will provide a procedure which is satisfactory when other more orthodox methods have failed.

I append tables of 1841 low corrections actually supplied out of 4000 eyes examined. As will be seen by reference to the strengths of the lenses, the errors were all within one dipotre of zero on either of the greater plus or minus meridians.

Tables of Low Corrections (4000 Eyes)

Table A. *Corrections (1 dioptre and under) used in Hyperopic and Mixed Astigmatism*

Lenses	$\frac{+0.25}{0}$	$\frac{+0.25}{-0.25}$	$\frac{+0.25}{-0.5}$	$\frac{+0.25}{-0.75}$	$\frac{+0.25}{-1.0}$	$\frac{+0.25}{-1.25}$			
Totals	67	68	52	21	5	5			
Lenses	$\frac{+0.5}{0}$	$\frac{+0.5}{-0.25}$	$\frac{+0.5}{-0.5}$	$\frac{+0.5}{-0.75}$	$\frac{+0.5}{-1.0}$	$\frac{+0.5}{-1.25}$	$\frac{+.05}{-1.5}$		
Totals	107	118	186	26	18	4	9		
Lenses	$\frac{+0.75}{0}$	$\frac{+0.75}{-0.25}$	$\frac{+0.75}{-0.5}$	$\frac{+0.75}{-0.75}$	$\frac{+0.75}{-1.0}$	$\frac{+0.75}{-1.25}$	$\frac{+0.75}{-1.5}$	$\frac{+0.75}{-1.75}$	
Totals	76	136	98	55	20	4	6	4	
Lenses	$\frac{+1.0}{0}$	$\frac{+1.0}{-0.25}$	$\frac{+1.0}{-0.5}$	$\frac{+1.0}{-0.75}$	$\frac{+1.0}{-1.0}$	$\frac{+1.0}{-1.25}$	$\frac{+1.0}{-1.5}$	$\frac{+1.0}{-1.75}$	$\frac{+1.0}{-2.0}$
Totals	85	83	160	40	33	11	12	3	9

Table B. *Corrections (1 dioptre and under) used in Myopic Cases*

Lenses		$\frac{0}{-0.25}$	$\frac{0}{-0.5}$	$\frac{0}{-0.75}$	$\frac{0}{-1.0}$
Totals		44	64	30	18
Lenses	$\frac{-0.25}{0}$	$\frac{-0.25}{-0.25}$	$\frac{-0.25}{-0.5}$	$\frac{-0.25}{-0.75}$	
Totals	22	28	10	6	
Lenses	$\frac{-0.5}{0}$	$\frac{-0.5}{-0.25}$	$\frac{-0.5}{-0.5}$		
Totals	13	18	28		
Lenses	$\frac{-0.75}{0}$	$\frac{-0.75}{-0.25}$			
Totals	6	16			
Lenses	$\frac{-1.0}{0}$				
Totals	21				

TABLE C. *Totals of Low Refractive Defects occurring in 4000 Eyes examined*

Hyperopia	under 1·0 dioptre		335
Hyperopic astigmatism	,,	,,	342
Compound hyperopic astigmatism	,,	,,	635
Mixed astigmatism	,,	,,	205
Compound myopic astigmatism	,,	,,	106
Myopic astigmatism	,,	,,	156
Myopic	,,	,,	62
			1841
Corrections over 1·0 dioptre			2159
			4000

DISCUSSION

Mr W. A. Dixey: I venture to suggest that to assume that many nervous disorders arise from the unconscious effort to correct small refractive defects is physiologically a large assumption, and that the onus of proof lies with the assumer. It is especially difficult to accept in the case of small spherical errors, for in this connexion it should be remembered that the zero point of refraction is not a critical point in the strict sense, and in any case the possible causes of discomfort should be examined. In the art of sight-testing there are always three functions of the eye to be considered: refraction, accommodation, and convergence. I always think of convergence as the Cinderella of the trio. Much, sometimes perhaps overmuch, is found in text-books on the first subject. Accommodation is generally adequately dealt with, but the problems of convergence are lightly regarded; and yet, I venture to think, it is more often to anomalies of convergence than to any other cause that the discomforts and *malaise* referred to are due. Mr Aves refers to trouble caused by direct errors of convergence, but there are other more subtle conditions that have to be considered. Interference with the normal relation of accommodation and convergence may well account for discomfort, and in this connexion the formation of an abnormal habit is important. For example, consider the case of a myope of three dioptres. He will have contracted the habit of converging up to 3 metre-angles without any effort of accommodation. Is it to be wondered at that, in view of the fact that the two functions are mutually stimulating, he should feel discomfort till he can adapt himself to the new conditions? The same point could be illustrated by a case of hyperopia and, better still, by one of presbyopia.

Mr H. C. Raxworthy: There are many interesting points raised in Mr Aves' paper that are well worth very careful study. As he remarks, there is very little published matter relating to the correction of low errors of visual defect. The point that struck me most forcibly was the one referring to the smooth changing of the trial lenses when testing. I am unaware of the device referred to in which a variable spherical lens is used, but I certainly think that a disc of lenses rotating before one eye of the patient is better and speedier than the cumbersome trial

case. Such a disc is in use on the Continent and in America with very beneficial results, and will, I believe, eventually supersede the ordinary methods generally adopted here. I know it is a difficult thing to produce, but if through the co-operation of the manufacturer and the refractionist a compact device could be evolved on the lines of the variable prism (which can give prism from zero to 30°), quicker and more accurate findings would be obtained.

The reason why the Maddox groove often shows an error of 1/4 or 1/2 Δ is that the plano lens in which the groove is formed has surfaces which are not quite parallel.

Mr E. F. Fincham: It appears that one of the greatest difficulties in the estimation of low errors of refraction is the determination of the absolute point of emmetropia. Probably the true static refraction of the eye can only be measured when the ciliary muscle is completely paralysed by a cycloplegic. This, however, is not the normal condition and it is found in practice that for distant vision the muscle should not be deprived of its natural "tone."

It would, I think, be helpful if Mr Aves could give us a little more information on the binocular method of testing of which he has spoken. The advantages of the method when it can be applied as for instance toward the end of the test are well known. The patient's accommodation is certainly more readily relaxed in binocular vision, but it seems that the method would be difficult in cases of anisometropia or even when errors of the same type but of different degrees occur in the two eyes—a condition most frequently met with.

Mr W. Swaine (communicated): Mr Aves' paper contains some interesting suggestions, among the most interesting being those concerning fine adjustment in muscle testing. In the case of his own correction there is a very definite observation recorded. In order that he may tolerate his correction he finds he must decentre one lens in a manner which bears no relation to the accepted muscular tests. This appears to me of primary importance and its investigation both on individual cases and on a statistical basis a matter of some urgency in refraction work. Has Mr Aves looked out for such cases and obtained records of the refractive errors, ordinary muscle findings, and the finer muscle tests? If so, it would be valuable to receive particulars.

I am not altogether convinced that nature has decreed for hyper-astigmatism *not* to be more frequent than mixed. I rather agree with Sheard and Gullstrand that the average imposed by nature is hyperopia, even without cycloplegia, and not emmetropia.

Mr T. Smith: I have listened with much pleasure to Mr Aves' paper, which brings back to my mind memories of the papers that formed so prominent a feature of this Society's work when I first became a member. The reading of these papers and the subsequent discussions were found most helpful in those days, and indeed were the immediate purpose for which the Society was founded. I very much hope that we shall have many such papers and discussions in the time before us.

It is interesting to compare the precision in corrections which Mr Aves advocates with figures based upon the physical properties of light. If light passes through an aperture of diameter δ millimetres, a change in the curvature of the wave-front will be barely perceptible until it exceeds $\pm 1/\delta^2$ dioptres. These limits also determine the accuracy of any experiment, whether objective or subjective, for finding the correction needed. For example, with a pupil 1 mm. in diameter, the uncertainty is 1 D: with a pupil of 4 mm. diameter, it falls to 1/16 D. These limits may be transferred from the pupil to the lens with sufficient accuracy, and we are led to ask why, if an accuracy of 1/8 D is needed when the errors are small, a very different degree of accuracy is accepted when the whole correction is large. There seems no physical justification for the difference.

When pronounced irregular astigmatism is present, I have sometimes wondered whether it would not be possible to give the patient a new outer surface to his cornea—I assume that irregularity here is the cause of the trouble. Prof. von Rohr has described the effect of wearing lenses in contact with the eye, but the surface I have in mind would become part of the eye itself, by the permanent attachment of a thin covering of a material like collodion which could be given a regular smooth external surface while on the other it was moulded by the eye itself. If a material suitable for the substance of a thin capsule should be unusable owing to surface effects, it might be possible to cover its outer surface with some thin membrane not open to this objection. I should be interested to know whether the difficulties of treating the eye in such a way are quite insuperable.

Mr H. H. Emsley: Mr Aves is to be congratulated on bringing forward the first paper of this kind for many years and, moreover, one that raises so many interesting questions. The only part of the paper on which I would like to interpose a word is that concerned with irregular astigmatism. Mr Aves states that it is comfort that has to be sought and not a high scientific precision. I do not think the attitude, which is frequently adopted, of assuming that comfort and high precision in testing are opposed is fair until at any rate more experimental work has been done. It may be that a very careful optical test will lead to comfort. Refractionists, in testing for astigmatism, try so hard to determine accurately *the* axis direction—assuming that the astigmatism is regular and that therefore the other principal meridian is at right angles—that they must usually miss the variations of refraction from one meridian to another, which variations are so often irregular. If this irregular variation were taken into account it might result in the axis direction of the cylinder finally prescribed being different from that found by the ordinary routine method of testing for astigmatism. It was so in my own case as described in a recent paper* to this Society.

Mr Aves: I willingly accept Mr Dixey's suggestion that the assumption that many nervous disorders are attributable to uncorrected refractive defects needs proof. I have indicated in my paper that many writers have given specific cases but if more convincing proof is required I would refer him to a paper which I

* *Trans. Opt. Soc.* 27 (1925–26) 28.

hope to read before the Optical Convention in April. There he will find a large group of such symptoms treated and reported on. I admit that things which are concomitant are not necessarily cause and effect, but where thousands of cases are treated by low corrections and relief of symptoms reported in the very large majority of cases (over 85 per cent.) the necessary proof is provided.

I agree also that defective or imperfectly adjusted convergence is a prolific cause of nervous disturbance but I include suitable prism corrections of low order as part of my subject, it being dealt with in note 4.

Mr Raxworthy's suggestion of a disc for smooth changing of lenses is a good one but I think such discs would be more useful if the lenses on the discs were sector shaped and fitted edge to edge rather than with metal separation. A still better method would be the use of continuously variable multifocal lenses which I proposed in my patent No. 15,735 (1907) but was unable to have produced owing to mechanical difficulties in grinding.

Mr Fincham raises a most interesting point which touches closely upon the fundamental principle underlying the system of testing I propose. He mentions the difficulty of determining what is the absolute point of emmetropia. I have suggested that there is something more to consider than mere optical exactitude and that is normality of "feeling." The successful refractionist aims not at correcting to an emmetropic normal but rather, if I may coin a word, to a "psychoptic" normal. That is to say, it is not merely getting a correction of the optical system of the eye but a satisfactory mental correlation which is necessary. I submit that the advantage of relaxing accommodation by binocular testing rather than by a cycloplegic is that the psychological element is preserved from unnatural paralysis by the former method. For research purposes, where the two factors require separate investigation, the cycloplegic is useful, but in general refractive practice it is a stumbling-block. Where anisometropia is present suitable relative adjustment between the two eyes is obviously necessary as a preliminary step.

Mr Swaine's communicated criticism I much appreciate. In reply I may state that the curious anomaly of my own muscular imbalance has led me to look for similar instances and I find they are extremely common. Indeed, I think it is probable that the great difficulty in fitting prisms is due to insufficient investigation of each case. Closer investigation will show that the various forms of heterophoria vary considerably when measured in different planes, whereas the usual one measured is the horizontal. For working conditions the planes 45° to 60° below the horizontal are mostly used. I also agree that Sheard and Gullstrand are probably right in assuming that a slight hyperopia is the normal healthy condition of the human eye. This is precisely what we should expect in the evolution of man as a hunter. Modern civilisation, however, has altered the environment and made close work the normal, and it is this change and the lack of the means of quick adaptation to the new conditions which make emmetropia or even slight myopia an advantage to many workers. There is no gainsaying the fact that the correction of low errors of hyperopia is a decided help to many workers whose reserve of nerve energy is lower than it should be.

Mr Smith's remarks on the effect of the size of the aperture in optical experiments and of the corresponding effects of varying pupillary widths is extremely interesting. It explains, on the optical side, to refractionists the curious phenomena of the differing sensibilities of various patients to an incorrect "correction." There is, however, another side and that is the psychological factor, as to why less attention is paid to the exact correction of high defects as compared with errors of low degree. With the highly defective patient "half a loaf is better than no bread." But Mr Smith is quite right in assuming that the careful correction of higher defects is desirable, on the superior grounds that the "whole loaf is even better than the half."

Mr Emsley aptly fills in the blanks of my paper on the effects of irregular astigmatism. His research work on his own condition should be extremely useful to refractionists. I can only say that my own eyes are the same, only more so! I would not go so far as to say that "comfort and high precision in testing are opposed"; I would rather put it that, inasmuch as it is impossible to correct irregular astigmatism by even a specially figured lens in more than one position of the eye, the refractionist has to be careful that he does not correct some part of the irregular optical system which may introduce fresh and unaccustomed action of the extra-ocular muscles. In my own case I find that the correction of some areas of the cornea introduces quite new hyperphorias. This is owing to the very unsymmetrical formation of the caustics in the vertical meridians.

In conclusion I would like to thank the members for their very kindly reception of a very inadequate paper and to assure them of the value of such meetings to all practising refractionists. I sincerely hope that the ophthalmic section of the Optical Society will afford a widely recognised opportunity for stimulating research on all problems pertaining to refraction.

VERTEX POWER AND ITS MEASUREMENT

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ABSTRACT. The object of the paper is to explain in a simple manner some of the problems involved in the designation and measurement of the power of ophthalmic lenses. The various ways in which the power may be expressed are discussed and the existing methods of measuring power are described, with special reference to the measurement of vertex power. An instrument for this purpose is described and the results of measurements on a number of lenses are given.

Introduction

With the demand for increased accuracy in the determination of refractive errors is coupled the need for accurate specification and measurement of the powers of the correcting lenses. So much has been written within recent years on the various methods of specifying the power of ophthalmic lenses that it would hardly seem necessary to add anything further to the subject; but considerable confusion seems to exist in various quarters as to the precise meaning of the terms involved, especially with regard to the varying results that are obtained by the different methods used for determining these values.

The purpose of this paper is to discuss the several methods that are commonly available for the measurement of the powers of ophthalmic lenses, with special reference to that for obtaining the so-called *vertex power*, or *vertex refraction*.

Methods of Expressing Power

The effect of an ophthalmic lens may be expressed in a number of different ways; we have the following quantities in relation to it:

(1) Equivalent or true power.

(2) Vertex power or vertex refraction—i.e. the power at the vertex nearer the eye.

(3) Neutralising power—usually the front vertex power.

(4) Thin lens power—i.e. the sum of the powers of the two surfaces, thickness being neglected.

The differences in these values in cases of high power lenses, or in lenses of widely differing form but of the same thin lens power, may be considerable and it is necessary therefore that the value stated in any case should be clearly defined.

(1) *Equivalent Power*

Equivalent power may be defined as the reciprocal of the distance in metres of the principal focus from the principal point. This quantity is seldom obtained in practice owing to the difficulty of determining the position of the principal point, but when the axial thickness of the lens is small, as in the case of negative lenses, the equivalent power differs little from the power expressed in other ways.

In the Optical Society's standards for trial cases the negative lenses are expressed in terms of their equivalent powers.

(2) *Vertex Power*

The front or back vertex powers will be the reciprocals of the distances of the first and second principal foci from the front and back vertices of the lens respectively. As the positions of the principal points will vary with the thickness and form of the lens, it follows that with lenses having the same equivalent power but of different forms the positions of the principal foci with respect to any point on the lens will differ; thus, if the equivalent power of the lens is kept constant while its

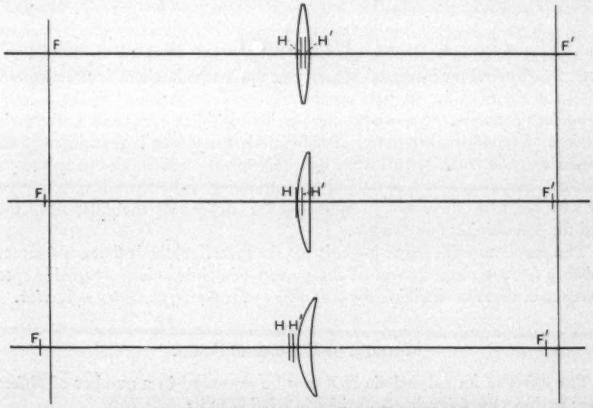

Fig. 1. *H* and *H'* principal points. *F* and *F'* principal foci

form is altered, the vertex power changes. This is illustrated in Fig. 1 which shows the positions of the principal foci and principal points of a + 10 D lens in the form of (*a*) an equi-convex, (*b*) a plano-convex, (*c*) a deep meniscus.

To correct the refractive error of an eye it is necessary that the second principal focus of the correcting lens shall coincide with the far point of the eye; thus, no matter what form the final correcting lens may have, it is essential that its secon principal focus shall occupy the same position with respect to the eye as that of the

lens or lenses used in making the test. In order that this shall be so, it is necessary that the position of the principal focus shall be specified with respect to some easily determined point on the lens and that the lens be placed at some definite point in relation to the eye. To ensure this the system of numbering lenses in terms of their back vertex powers was introduced.

Any lenses of equal vertex power, although of different form or thickness, placed so that their back vertices occupy the same position in respect to the eye will have their second principal foci in the same position and are therefore interchangeable. Thus it will be seen that in the case of ophthalmic lenses vertex power is the important quantity as it gives the effective value of the lens at some definite point with respect to the eye.

(3) *Neutralising Power*

The most usual method of determining the power of an ophthalmic lens at the present time is to neutralise it. This consists in finding a lens of known power and of opposite sign which when placed in contact with the lens under test gives a combination such that no movement of an object viewed through the system is apparent as the combination is moved from side to side. The usual practice in neutralisation is to place the trial case lens in contact with the front surface of the

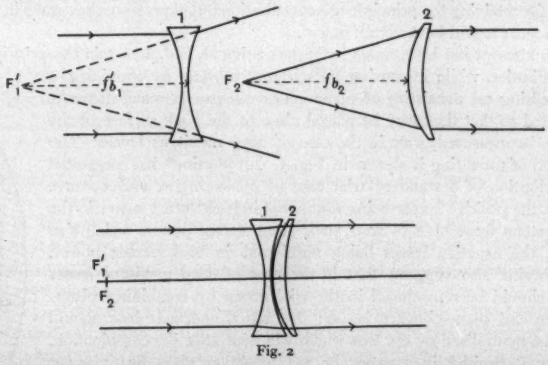

Fig. 2

lens whose power is required; and therefore in order to effect neutralisation it is necessary that the back vertex power of the neutralising lens shall be equal but of opposite sign to the front vertex power of the lens being measured. The explanation of this is shown in Fig. 2; lens 1 is the negative trial case lens having its second principal focus at F_1' and lens 2 is the positive lens whose power is required and having its first principal focus at F_2. If the distances f_{b_1} and f_{b_2} are equal, then, when the lenses are placed in contact as in the lower figure, F_1' and F_2 coincide

and the power of the combination is zero; thus the distance f_{b_2} and therefore the front vertex power of the positive lens has been obtained in terms of the power of the negative trial lens.

In the Optical Society's standards for trial cases the negative lenses are marked in terms of their equivalent powers and owing to the small central thickness (1 mm.) these powers agree very closely with their vertex powers. The positive lenses are marked in terms of the negative lenses which they will neutralise, i.e. their vertex powers. In neutralisation, therefore, one obtains the vertex power of the lens tested referred to the vertex against which the trial case lens is placed; in practice, as has been previously stated, this will usually be the front vertex power as in the case of periscopic and meniscus lenses it is not possible to place the test lens in contact with the back surface.

Neutralisation as a means of obtaining the power of an ophthalmic lens possesses a number of advantages over other methods; it is comparatively easy and can be carried out with considerable accuracy by persons with little technical training, and it requires no apparatus beyond a trial case which is a necessary part of an optician's equipment, while its principle, at least in its main outlines, is easily understood. Another advantage of neutralisation is that, in making the test, defects, such as the waviness of surface, sometimes met with are more readily detected than when other methods are used. For these reasons various suggestions have been made for retaining the principle of neutralisation but expressing the results in terms of the more useful back vertex power.

An attempt has been made by Messrs Stigmat, Ltd., to retain the principle of neutralisation while measuring back vertex powers, by providing a neutralising set consisting of plano-concave lenses of small diameter mounted so that they may be placed close to the back surface of the lens to be measured, even in the case of deep meniscus lenses. The method of mounting is shown in Fig. 3. Mr Swaine* has suggested the adoption of a standard trial case of plano-convex and concave lenses, the positive lenses being numbered in back vertex power in the usual steps from 0·12 to 20 D, their front vertex powers being also given, the negative lenses being numbered in back vertex power. Mr Swaine also suggests that in the case of deep meniscus lenses these should be reproduced in the trial frame by combining plano-convex and plano-concave lenses; the actual spectacle lens should then be neutralised by the lens which will neutralise the combination.

Fig. 3

Mr T. Smith† in a paper on neutralisation gives formulae for obtaining the back vertex power from the power found by neutralisation. Tables giving the relative values of neutralised and vertex powers have also been published‡. With each of these suggestions it would seem, however, that, while the neutralising method has been retained, much of its particular value—its simplicity—has been lost.

* W. Swaine, "A Standard Trial Case," *Trans. Opt. Soc.* 25 (1923–24) 73.

† T. Smith, "The Theory of Neutralization," *Trans. Opt. Soc.* 26 (1924–25) 38.

‡ O. Henker, *Introduction to the Theory of Spectacles* (1924).

(4) *Thin Lens Power*

A common and simple method of determining the power of an ophthalmic lens is by means of the lens measure, a simple spherometer divided to read the power of the lens surface for a given refractive index (1·523), the power of the lens being expressed as the sum of the two surface powers. The method obviously takes no account of the differences in power produced by changes of thickness and form of the lens and the values so obtained may differ from those expressed in the ways previously mentioned. Other drawbacks of this method are that the lens measures are often mechanically unreliable; their scales are a compromise and the refractive index of the glass may not be that for which the instrument is divided. Lenses of glass other than spectacle crown are now being made*.

Table I shows the values of thin lens, equivalent, neutralising, and vertex power for a number of lenses.

Table I

Surface power	Central thickness	Thin lens power	Equivalent power	Neutralising power	Vertex power
Dioptres	mm.	Dioptres	Dioptres	Dioptres	Dioptres
+ 3·0, 0	2·5	+ 3·0	+ 3·0	+ 3·0	+ 3·005
+ 1·5, + 1·5	2·5	+ 3·0	+ 2·996	+ 3·01	+ 3·01
+ 9·0, − 6·0	2·5	+ 3·0	+ 3·10	+ 3·07	+ 3·16
+ 6·0, 0	3·5	+ 6·0	+ 6·0	+ 6·0	+ 6·08
+ 3·0, + 3·0	3·5	+ 6·0	+ 5·98	+ 6·02	+ 6·02
+ 12·0, − 6·0	3·5	+ 6·0	+ 6·17	+ 6·08	+ 6·34
+ 12·0, 0	5·5	+ 12·0	+ 12·0	+ 12·0	+ 12·54
+ 6·0, + 6·0	5·5	+ 12·0	+ 11·87	+ 12·14	+ 12·14
+ 18·0, − 6·0	5·5	+ 12·0	+ 12·39	+ 12·12	+ 12·95

Measurement of Vertex Power

In the case of a positive lens the vertex power may be obtained by making a direct measurement of the distance of the second principal focus from the back vertex of the lens. This method will, however, not be practicable in the case of lenses of low power owing to the great distance between lens and focus, while in the case of negative lenses we have no simple means of finding the position of the virtual focus.

During recent years a number of instruments for measuring vertex power have been devised and placed on the market. These instruments give the vertex power from one reading and are all constructed on the same principle.

* S. A. Emerson, "Some Recent Improvements in Modern Ophthalmic Lenses," *Trans. Opt. Soc.* 27 (1925–26) 1.

Principle of Instrument for measuring Vertex Power

The optical arrangement of the instrument is shown in Fig. 4. The lens L to be measured is placed with its back vertex exactly at one of the principal foci of a standard lens S, and a target T is moved until the light from it emerges parallel from the lens S. This position is found by receiving the light in a telescope which has been focussed for a distant object.

The principal foci of the standard lens S are at L and F respectively, and for an object at T a virtual image is formed at T' by the standard lens. If, now, we call the distance FT of the object from the first principal focus x, and the distance LT' of the image from the second principal focus x', we have from the well-known Newton's relation

$$xx' = f^2_S, \text{ where } f_S \text{ is the focal length of the standard lens } S.$$

Thus $$x = \frac{f^2_S}{x'} \qquad \text{.....(1).}$$

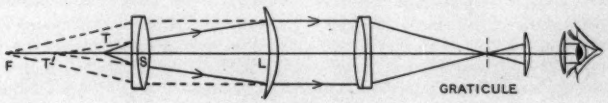

Fig. 4

The distance LT' or x', as will be seen from the diagram, is the vertex focussing distance of the lens being measured, the reciprocal of this distance being the vertex power F_V.

From (1) we have therefore

$$x = FT = \frac{F_V}{F^2_S},$$

where F_V is the vertex power of the lens being tested and F_S the power of the standard lens and therefore a constant.

When the lens L is removed, the target will be sharply focussed when at F and its movement FT, in order to re-focus it when the lens is inserted, is proportional to the vertex power of the lens. This movement may therefore be read on a scale divided to represent dioptres of vertex power. As an example, if the standard lens have a power of + 10 D, then $FT = \frac{F_V}{10^2}$ and for each dioptre vertex power of the lens under test the movement of the target will equal $\frac{1}{100}$ metre = 1 cm. When the lens L is negative the target will have to be moved out beyond F. In the example given above it would only be possible to measure positive lenses of powers up to 10 D, as for this value the target will come into contact with the standard lens. In order to increase the range of powers that can be determined it is necessary to increase the power of the standard lens; thus, if the standard lens have a power of 20 D, each dioptre of vertex power is represented by a movement

of the target of $\frac{1}{20^2} = \frac{1}{400}$ metre = 2·5 mm. and for a positive lens of + 20 D vertex power the target will then be in contact with the standard lens.

The large range of powers which have to be dealt with in ophthalmic lenses, varying from 1/8 to 20 D, introduces mechanical difficulties in the design of the instrument, for while the target movement for each dioptre of power must be small enough to allow of powers up to + 20 or so dioptres being measured, the movement in the case of low powers of fractions of a dioptre must be appreciable so that they may be read with some considerable accuracy.

The first instrument on this principle was the Vertex Refractionmeter of Zeiss which was protected in 1913. In the original specification* the arrangement of the instrument departed somewhat from the description given above. In place of the movable target viewed through a telescope a collimator with an illuminated object was used in place of the telescope and the image formed by the standard lens at the position T in Fig. 4 was observed by a microscope, the movement of the microscope being read on a scale divided to give dioptres. The instrument has now been put on the market in the modified form as described with the moving target.

In 1917 a patent† was taken out by Trotter for the instrument as now generally made, the principle of which has been described above.

Description of Instrument

The general construction of the instrument is shown diagrammatically in Fig. 5. The telescope T is fitted with a focussing eyepiece and a graticule at the focal plane of the objective; the lens L to be measured is held with its back vertex against a stop which must be accurately positioned with respect to the standard lens S. This lens, on the power of which the accuracy of the instrument depends, is usually constructed as a combination of a positive and negative lens in order that by an alteration of the separation of the two components its power may be adjusted to agree with the dioptre scale of the instrument. The target T', illuminated by a lamp, is moved by means of a rack and pinion along the axis of the instrument. As has been previously pointed out, the movement corresponding to each dioptre of power is small when the range of the instrument is large and in most cases a large divided head is attached to the pinion in order to obtain a sufficiently open scale.

The target is rotatable so that its lines may be brought parallel to the principal meridians of the lens when this is cylindrical or sphero-cylindrical in power. The powers in the principal meridians are then obtained by two settings of the target and the directions of these meridians read on a protractor attached to the instrument.

The chief drawback of the instrument, as at present constructed, apart from its expense, is the fact that its accuracy depends on the correct focussing of an image in a telescope. Unless the observer has had considerable experience, focussing, even when a graticule is provided, is apt to prove unreliable. A further source of

* *Brit. Pat.* 12,100. † *Brit. Pat.* 120,984.

error likely to occur in the instrument in course of time is that of backlash in the focussing mechanism.

Table II

Marked power	Measured vertex powers		Remarks				
	Back	Front					
Dioptres	Dioptres	Dioptres					
+ 6·0	+ 6·12	+ 5·80	British	− 6·0 D base			
+ 4·0	+ 4·12	+ 3·98	do.	do.	do.		
+ 7·0	+ 7·12	+ 6·87	American	− 4·0 D	do.		
+ 4·0	+ 4·05	+ 3·98	do.	− 6·0 D	do.		
+ 4·0	+ 4·03	+ 3·90	French	− 6·0 D	do.		
+ 5·50	+ 5·65	+ 5·48	do.	− 6·0 D	do.		
+ 6·0	+ 6·10	+ 5·81	do.	− 6·0 D	do		
+ 3·00	+ 3·00	+ 2·90	German	− 6·0 D	do.		
+ 6·00	+ 5·93	+ 5·67	British	− 6·0 D base, marked in vertex power			
+ 6·75	+ 6·65	+ 6·45	do.	− 3·0 D	do.	do.	
+ 7·00	+ 6·90	+ 6·68	do.	do.	do.	do.	
+ 4·00	+ 4·00	+ 3·82	German	− 7·0 D base, marked in vertex power			
+ 4·50	+ 4·55	+ 4·40	do.	+ 8·0 D	do.	do.	Toric
+ 4·75	+ 4·80	+ 4·65					
− 9·0	− 9·0	− 9·0	do.	+ 3·0 D	do.	do.	
− 10·0	− 10·20	− 10·10	do.	+ 2·5 D	do.	do.	

Table II shows the results of measurements of front and back vertex powers for a number of meniscus lenses.

The actual instrument used in taking these measurements was the Vertometer of Messrs Bausch and Lomb, which is constructed on the principle just described; the dioptre scale of this instrument is divided to $\frac{1}{8}$ D up to $\pm$ 3 D and above that to $\frac{1}{4}$ D, and a subsidiary scale divided to $\frac{1}{10}$ D was used to obtain a more exact reading in the case of the results shown. The reading of the instrument was checked at various points on the scale with lenses whose vertex focussing distances had been carefully determined on the optical bench. In all cases the lenses were obtained from stock from well-known manufacturers and were not specially selected.

Except in those cases in which the lens is nominally marked in vertex power an examination of the results seems to show that something in the nature of a compromise between vertex and neutralising power is aimed at, as in every case the neutralising power is less than the marked value although in most cases the difference between the marked and the neutralising power is small and considerably less than the 2 per cent. allowed by the report of the Optical Society Standards Committee on Trial Cases. It would be of interest if manufacturers could give some information as to the methods by which they arrive at the curves, etc., of their lenses.

Advantages of the Vertex Power System of numbering Lenses

The advantages of expressing and measuring lenses in vertex power may now be stated. As has already been explained, vertex power expresses the effective power of the lens at some definite and easily determined point on the lens and therefore lenses of any form having the same vertex power are interchangeable if the

vertex of each lens occupies the same position with respect to the eye; usually this position is such that the lens just clears the eyelashes, i.e. a distance of 12 mm. In sight-testing the correction is usually found as the combination of two lenses in the trial frame; the vertex power of this combination may be measured, say, with the instrument just described and the final correcting lens can be made to give the

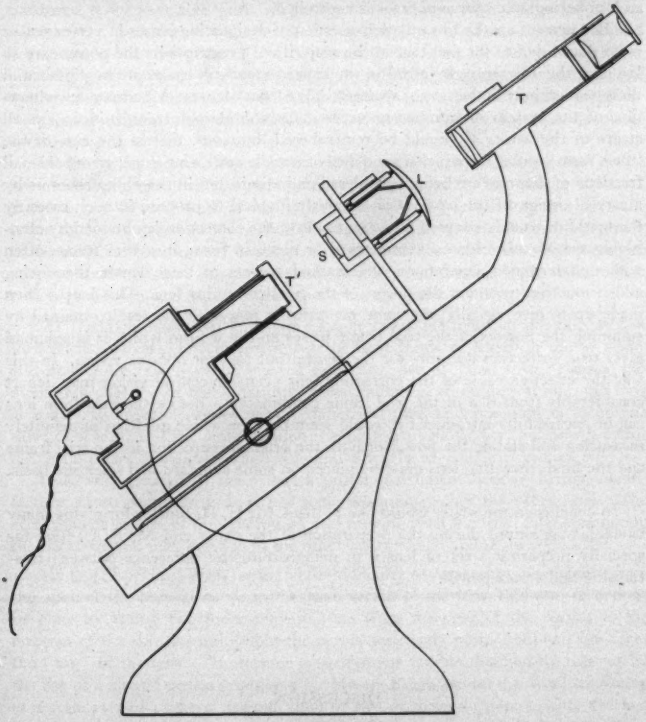

Fig. 5

same vertex power with the accuracy provided by the usual steps in power at present available in commercial practice. Many of the cases of dissatisfaction and discomfort with the finished lenses which are experienced at the present time would no doubt be eliminated by ensuring that the vertex power of the finished lens agrees with that of the trial frame combination.

While it is realised that any system of sight-testing and correction as at present practised must necessarily be of a more or less approximate nature, it is advisable that any error that can be controlled should be eliminated or at least reduced to known negligible limits. By reproducing the prescription in the finished lens to a known degree of accuracy, as may be done by adopting the vertex power system of numbering, one source of error is controlled.

The present apathy towards this question of designating lenses in vertex power is no doubt due to the fact that in the majority of prescriptions the powers are so low that the differences involved in the case of meniscus lenses are negligible and disappear entirely in the case of symmetrical or "flat" lenses. Moreover, any alteration of the system of numbering necessitates considerable reorganisation at all stages of the work. It should be remembered, however, that in the case of the "best form" lenses, on which a good deal of work is now being done, errors of small fractions of dioptres are being considered and eliminated in the oblique beams by a careful design of the lens. It is obviously illogical to proceed in such cases by the methods usually adopted at present, where the correction is obtained by combining two or more lenses, symmetrical or plano in form, in a trial frame, often with undetermined separations, the marked powers of these lenses then being added together to obtain the power of the final correcting lens. This lens is then made up to give, usually, the same neutralising power as the result obtained by summing the powers of the trial frame lenses and in a form which it is assumed gives true correction not only for the central but also for oblique vision. In this way the effective value of the correcting lens even for central vision may depart considerably from that of the trial frame combination. Before the best form lens can be successfully introduced it would seem that the whole question of definitely measuring and stating the power of both the original correction in the trial frame and the final correcting lens must be placed on some standard and scientific basis.

In conclusion the writer would like to thank Mr H. H. Emsley for giving many valuable suggestions during the preparation of the paper and Mr R. C. Reid for specially preparing a set of lenses to demonstrate the difference between neutralising and vertex power.

DISCUSSION

Dr John S. Anderson: As the registering mechanism of the instrument described in the paper depends on a simple rack and pinion movement, to what extent are the results likely to be affected by backlash? The British instrument exhibited does not suffer from this defect, as the scale is more open and does not therefore require a magnifying device. What is the reason for using a red filter in this instrument? It would surely be better to use a green filter.

Mr T. Smith: In taking measurements of the vertex power of lenses by the instrument described, I should anticipate a gradual loss of accuracy with use. To avoid this a scale on the sliding part read by a vernier would seem preferable to a scale carried by the actuating disc.

The measurement of powers from a fixed part of the lens has everything to commend it for spectacle work. With fixed base curves what this point is does not matter. The vertex has the merit of simplicity, but from observation I should have thought that the proximity of the rim with the brow the determining factor in practice rather than the vertex and the eyelashes. Reference to the rim—of course of specified diameter—would appear more convenient in routine measurements, as by the instrument described. Theoretically one choice is as simple as another.

In discussing neutralisation in an earlier paper, some formulae had to be introduced to present an acceptable theory of the process. The course indicated then was not the derivation of the back vertex power by calculation after neutralisation of the front vertex power, but direct neutralisation of the back vertex power of the positive by a negative lens having a front surface which fits the base curve adopted for the positive lenses. The only difficulty which arises is due to distortion with high powers, and with a little experience this difficulty disappears.

Mr H. H. Emsley: Mr Fincham's paper raises matters that are not only of theoretical interest. The increasing use of bent forms of lenses in recent years, and now the probably early, even if restricted, use of "best form" lenses, has made this question of the suitability of neutralisation an important one. So long as nearly all spectacle lenses were symmetrical (flat) and precision in technique was not of a high order, neutralisation was perfectly satisfactory, but now it introduces a number of anomalies and cumulative errors, as ordinarily carried out.

I am very pleased to see a vertex power instrument here of British manufacture, more particularly as it has some advantages over the other ones. The arrangement results in the reading pointer moving with the lens, thus eliminating the backlash that may develop in the other instruments, and in an open scale.

Mr R. J. Lucking: With regard to the difficulty of measuring the distance from the apex of the corneal curve to the back vertex of the lens, this can, of course, be done by taking two measurements, one from the apex of the cornea to the position of the rim (assuming that the rim is accurately positioned) and the other the "sag" of the lens. The former measurement can be obtained by rule, or by the use of a simple device (marketed by Messrs Busch before the War) consisting of a scale viewed under a magnification of two or three diameters, with which it is quite easy to read off to 0·5 mm. The depth of the "sag" of the lens can easily be obtained by the use of any ordinary depth gauge such as instrument-makers use, or by calculation.

Mr Fincham: As is pointed out in the discussion and mentioned in the paper, backlash in the rack and pinion movement of the instrument as usually constructed will introduce serious errors, especially as the movement of the target is

so small. In the new British instrument exhibited, which was received too late for a description to be included in the paper, the method described in the original Zeiss specification has been adopted with the difference that the observing microscope is fixed and the rest of the apparatus travels with respect to it. In this way, as Dr Anderson and Mr Emsley point out, a more open scale can be used with a direct reading of the moving part of the apparatus. There would seem to be no reason for the adoption of a red filter in preference to the green.

I hardly think the use of a vernier as suggested by Mr Smith would be suitable in an instrument of this type for commercial work.

While the proximity of the rim of the lens with the brow is the determining factor in the position of the lens in some cases, I think the more usual practice is to arrange that the lens just clears the eyelashes. Mr Smith's interesting suggestion of making the rim the reference position from which powers are measured, while overcoming the difficulty of observing the position of the vertex with reference to the eye in the case of meniscus lenses, would be difficult to apply in the case of ophthalmic lenses of varying dimensions and shapes.

In reply to Mr Lucking, it is doubtful whether many people will go to the trouble of making the two measurements he describes.

NOTES ON SOME PHYSICAL AND OPTICAL CHARACTERISTICS OF THE STYLUS-ON-CELLULOID METHOD OF RECORDING

By W. G. COLLINS

[Communicated by Mr R. S. Whipple, M.I.E.E.]

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ABSTRACT. It is shown that the stylus-on-celluloid record consists of a cylindrical depression forming a minute cylindrical lens, bordered by ridges forming quasi-cylindrical lenses of positive form and beyond these concave cylinders. When viewed with a properly focussed microscope, the record appears as a dark band on a bright ground and at the centre of the dark band is a fine bright line on which precise measurements can be made. The record is not made by scratching but by plastic deformation.

Records obtained by the above process have frequently been referred to as scratching on celluloid. It appears desirable, therefore, to explain more fully the physical and optical characteristics that have conduced to the successful employment of the principle in the design of several new types of recording apparatus. Celluloid, Celastoid, ebonite, and other composite materials are generally considered to be fairly perfectly elastic under compression, excepting when the temperature is raised considerably. The elastic limit is, however, fairly low, and plastic deformation begins at comparatively low pressures. In consequence, if a minute spherical point is used, a very small total pressure will give sufficient pressure under the stylus to cause plastic flow, and it is found that these substances flow with great readiness when the indentations are small, say, from 2 to 4 microns in depth. The deformation of the surface produced by this means is permanent and reproduces the form of a point with great fidelity when the spherical point moves over the celluloid. The record thus consists of a cylindrical depression of small radius, usually from 10 to 30 microns, forming a minute concave cylindrical lens having an approximate virtual focal length of 0·1 mm. In addition to this depression there are produced on each edge of the record ridges which form quasi-cylindrical lenses of positive form and of still smaller radius of curvature; their focal lengths approximately are only 0·03 mm. Each of these is further bordered by concave cylinders of slightly shorter focal length than that of the primary depression. These effects are shown diagrammatically in Fig. 1, where A represents the depression made by the spherically ended style, B, B are the ridges forming the positive quasi-cylinders, and C, C the two outer negative cylinders. The virtual foci of A and C, C are situated at a and c, c and those of B, B at b, b.

Actually, the foci *c*, *c* are more nearly in the same plane as *a*. In order to demonstrate these features a section of a record has been obtained and a photo-micro-

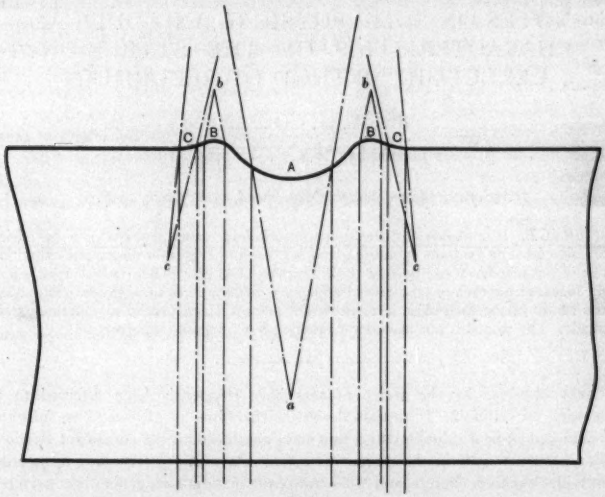

Fig. 1

graph given in Fig. 2, which shows clearly the cylindrical depression, the raised quasi-cylinders, and the bordering concave cylinders, the existence of the latter having previously only been deduced from the optical effects produced by the record.

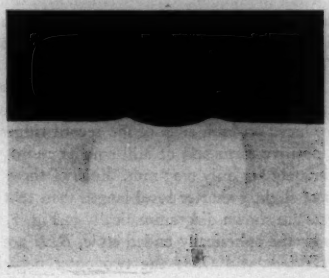

Fig. 2

Attention has previously been called to the fact that stylus-on-celluloid records, when viewed with a properly focussed microscope by transmitted parallel light, show as a comparatively broad and dark band on a bright ground and in the centre of the dark band appears a fine bright line, and this latter characteristic permits of the measurement of such records to a very high degree of precision. It will be seen that the microscope objective is focussed, not on any part of the deformed surface, but at the virtual focus of the concave cylindrical lens formed

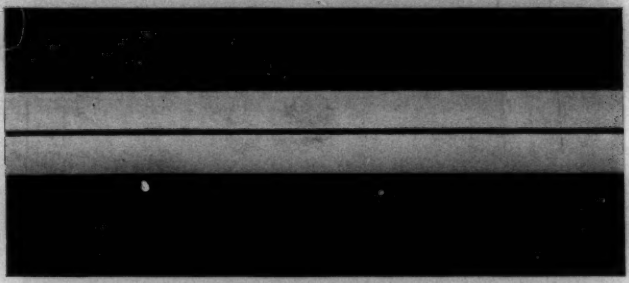

Fig. 3

by the spherical pointed style. The quasi-cylindrical ridges are not sharply in focus, but the lenses *C*, *C* (Fig. 1) have their foci in approximately the same plane as *A*, and thus tend to give considerable concentration of light along the edges, the effect of which is clearly shown in Fig. 3 in the increased density of the photographic image. This photograph also shows the fine central line previously mentioned as appearing visually bright. Fig. 4 shows the effect of focussing

Fig. 4

the microscope slightly within the virtual focus of the record and on the focal plane of the ridges *B*, *B*. Fig. 5 is a photomicrograph of a scratch showing very clearly the difference in the optical characteristics. The force required to produce a scratch is also enormously in excess of that required for the plastic deformation of the celluloid surface.

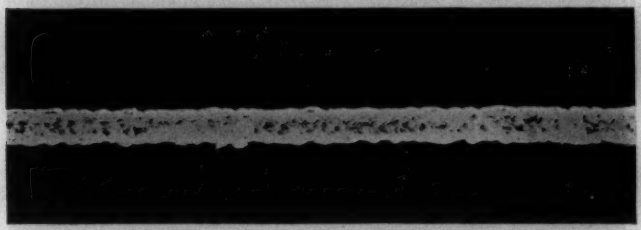

Fig. 5

REVIEWS

Fremdsprachliches Optisches Wörterbuch (*deutsch-englisch-französisch-spanisch*). Teil II: *Die Brille*. Herausgegeben von der Deutschen Optischen Wochenschrift. Bearbeitet von Dr W. PIETZSCH. Pp. vii + 149. (Erfurt: Deutsche Zeitschriften-Gesellschaft m. b. H.) Price R.-M. 5.

This book is the second volume of a series of seven technical vocabularies; the first, dealing with the eye, has been reviewed already (see *Trans. Opt. Soc.* 26 (1924–25) 204). The present volume contains the terms connected with the work of the practising optician and is exceedingly full and complete. A useful Appendix is the recent standard nomenclature recommended by the Technische Ausschuss für Brillenoptik ("Tabo"). An interesting example of the need for such a standard is the occurrence in the title of the word "Klemmerteile." Reference to the word "Klemmer" in the Vocabulary results in the enquirer being referred to "Pincenez." The book is very well compiled and fully worth its price.

J. W. T. W.

The Principles of Physical Optics. By ERNST MACH. Translated by John S. Anderson, M.A., D.Sc., Ph.D., and A. F. A. YOUNG, B.Sc., Ph.D. Pp. xi + 324; 280 Figs. and 10 Plates. (London: Methuen and Co., Ltd., 1926.) Price 21*s*. net.

Ernst Mach is well known in this country as a professor of philosophy at Vienna who devoted special attention to the foundations of physics and wrote on such subjects as the concept of force and relativity of motion. There is no branch of physics the fundamental concepts of which are in such a state of flux at present as Optics: the question of the ether, the problem of ionization, the nature of the quantum, the *J* phenomenon are all subjects which exercise the worker very seriously, and the reader turns towards the book under review which bears the sub-title of "An Historical and Philosophical Treatment" in the hope that it may help to clarify his difficulties. In this hope he will be quite disappointed. These subjects are not treated at all in the book, which was finished in 1913 and deals with ordinary physical optics, as it was understood twenty years ago, in a historical and semi-popular manner. To the physicist the treatment does not appear as searching as it might be. The author states in the preface that he discredits the theory of relativity and questions whether it will prove more than a transitory inspiration in the history of science, and he also assumes as a matter of course that light has no mass; his successor in the chair at Vienna, Dr Moritz Schlick, is a confirmed relativist.

But if the reader is disappointed in the book as a philosophical treatment of the subject he will be more than gratified by it as a historical treatise. This is a good thing from the publisher's point of view, as the public interested in the historical aspect of the subject is a larger one than the public interested in the philosophical aspect. We possess no modern work in English which describes the fundamental discoveries with such historical detail, with full references, and in such an interesting manner. Priestley's *History of Optics* was a book of the same class. The statement on the dust cover that the work will afford a useful and comprehensive survey for the student who is interested in the historical development of the subject and has neither the time to read the original works of Newton, Fresnel, etc., nor perhaps the faculty of estimating the true values of the various theories, is amply justified. It is not so much Newton and Fresnel as the

lesser known authors such as Grimaldi or Malus, for which the book will be valuable. The ten plates of the pioneers of the subject—of three of whom, Porta, Grimaldi, and Fraunhofer, I have seen no portraits before—together with the attractive production of the book render it one for all lovers of optics. The translation is well done and is of such a nature that except for four or five sentences the work might have been written in English.

No part of the work calls for special comment more than another, as the treatment is balanced and the author does not obtrude himself, but allows the different discoverers to speak for themselves. He is also guarded in his deductions. The book commences with the rectilinear propagation, Lambert's photometric laws, and the velocity of light. Chap. III deals with reflection and refraction, but the author has obviously not studied Kepler's work on the law of refraction in *Ad Vitellonem Paralipomena* at first hand. Then follow chapters on the early knowledge of vision and the development of dioptrics; Gauss's treatment of the theory of a coaxial system of lenses is very briefly considered. We have next an account of Newton's work on the spectrum with reproductions of the original diagrams from the *Opticks*. Only the first part of the *Lectiones Opticae*, p. 87, it should be noted, has been printed in English. With reference to colour vision it is interesting to note that Mach states, p. 106, that neither the number nor type of primary-colour sensations assumed by Young admit of an unbiassed analysis, also, p. 107, that Hering's theory not only corresponds with the most rational principles of research, but also conforms most accurately to all the known facts. It is characteristic of the treatment that no mention is made of the question as to whether the prism makes the colours or whether they are already in existence in the white light before it reaches the prism. Next follows an account of the development of the ideas of periodicity and interference, and then there are chapters on the polarization of light. The account of the work of Newton and of Malus in this connexion is very interesting. The last chapters deal with Huygens' principle and diffraction.

It will come as a surprise to English readers to find that the form of refractometer employed by Rayleigh, which consists of a collimator and telescope in line with two long parallel tubes between, had been used previously by Fresnel and Arago to determine the differences in refractive index of hot and cold air and of dry and moist air, also that Arago devised a compensator for use with the instrument for the purpose of keeping the path difference small. This was an anticipation of the later and more efficient arrangement devised by Jamin.

It is the side-lights of this nature thrown on the conventional treatment of the subject that constitute the most valuable feature of the book.

R. A. HOUSTOUN.